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PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCV.

PART I.

LONDON,

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ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1794, the Medal on Sir GODFREY COPLEY'S Donation, to Sig. ALESSANDRO VOLTA, Professor of Experimental Philosophy in the University of Pavia, for his several communications explanatory of certain Experiments published by Professor Galvani.

PHILOSOPHICAL

TRANSACTIONS.

I. *The* Croonian *Lecture on Muscular Motion. By* Everard Home, *Esq. F. R. S.*

Read November 13, 1794.

WHEN I had the honour last year of presenting an apology for the unfinished state in which Mr. HUNTER left the CROONIAN lecture, I laid before this learned Society the plan upon which he meant to proceed; but my mind was at that time unfitted to prosecute so arduous an inquiry.

The progress Mr. HUNTER had made in this investigation enabled him to prove the crystalline humour of the eye to be laminated, and the laminæ to be composed of fibres; but the use to which these fibres are applied in the œconomy of the eye he had not ascertained, although several experiments were instituted with that view: his opinion was certainly in favour of their being muscular, for the purpose of adjusting the eye to different distances by their contraction and relaxation.

Being unwilling that a subject on which Mr. Hunter had so publicly given his opinion should remain in an unfinished state, I requested the President's permission to be allowed to give the Croonian lecture for the present year, as it would afford me an opportunity of weighing with impartiality the facts already ascertained, and of endeavouring by my own labours to add to their number.

In prosecuting this inquiry, I consider myself to have been particularly fortunate in having had the assistance of my friend Mr. Ramsden. It was a subject connected with his own pursuits, and one which had always engaged his attention; he was therefore peculiarly fitted, both by his own ingenuity and knowledge in optics, for such an investigation.

In conversing upon the different uses of the crystalline humour, he made the following observations.

He said, that as the crystalline humour consists of a substance of different densities, the central parts being the most compact, and from thence diminishing in density gradually in every direction, approaching the vitreous humour on one side, and the aqueous humour on the other, its refractive power becomes nearly the same with that of the two contiguous substances. That some philosophers have stated the use of the crystalline humour to be, for accommodating the eye to see objects at different distances; but the firmness of the central part, and the very small difference between its refractive power near the circumference and that of the vitreous, or the aqueous humour, seemed to render it unfit for that purpose; its principal use rather appearing to be for correcting the aberration arising from the spherical figure of the cornea, where the principal part of the refraction takes place, producing the same effect

that in an achromatic object glass we obtain in a less perfect manner, by proportioning the radii of curvature of the different lenses. In the eye, the correction seems perfect, which in the object glass can only be an approximation, the contrary aberrations of the lenses not having the same ratio; so that if this aberration be perfectly corrected at any given distance from the centre, in every other it must be in some degree imperfect.

Pursuing the same comparison: In the achromatic object glass, we may conceive how much an object must appear fainter from the great quantity of light lost by reflection at the surfaces of the different lenses, there being as many primary reflections as there are surfaces; and it would be fortunate if this reflected light was totally lost. Part of it is again reflected towards the eye by the interior surfaces of the lenses, which by diluting the image formed in the focus of the object glass, makes that image appear far less bright than it would otherwise have done, producing that milky appearance so often complained of in viewing lucid objects through this sort of telescope.

In the eye the same properties that obviate this defect, serve also to correct the errors from the spherical figure, by a regular diminution of density from the centre of the crystalline outward. Every appearance shews the crystalline to consist of laminæ of different densities; and if we examine the junction of different media, having a very small difference of refraction, we shall find that we may have a sensible refraction without reflection: now if the difference between the contiguous media in the eye, or the laminæ in the crystalline, be very small, we shall have refraction without having

reflection, and this appears to be the state of the eye; for although we have two surfaces of the aqueous, two of the crystalline, and two of the vitreous humour, yet we have only one reflected image, and that being from the anterior surface of the cornea, there can be no surface to reflect it back, and dilute an image on the retina.

This hypothesis may be put to the test, whenever accident shall furnish us with a subject having the crystalline extracted from one eye, the other remaining perfect in its natural state; at the same time we may ascertain whether or no the crystalline is that part of the organ, which serves for viewing objects at different distances distinctly. Seeing no reflection at the surface of the crystalline, might lead some persons to infer that its refractive power is very inconsiderable, but many circumstances shew the contrary; yet what it really is may be readily ascertained, by having the focal length and distance of a lens from the operated eye, that enables it to see objects the most distinctly; also the focal length of a lens, and its distance from the perfect eye that enables it to see objects at the same distance as the imperfect eye; these data will be sufficient, whereby to calculate the refractive power of the crystalline with considerable precision.

Again, having the spherical aberration of the different humours of the eye, and having ascertained the refractive power of the crystalline, we have data from whence to determine the proportional increase of its density as it approaches the central part, on a supposition that this property corrects the aberration.

These observations of Mr. RAMSDEN respecting the use of the crystalline lens, I was very desirous of bringing to the proof;

and while my mind was strongly impressed by them, a favourable opportunity occurred. A young man came into St. George's hospital with a cataract in the right eye: this proved to be a fair case for an operation, to which the man very cheerfully submitted, and was put under my care for that purpose.

In performing the operation, the crystalline lens was very readily extracted, and the union of the wound in the cornea took place unattended by inflammation, so that the eye suffered the smallest degree of injury that can attend so severe an operation; these circumstances it is proper to mention, as they contributed to render the patient a more favourable subject for experiment.

The man's name was BENJAMIN CLERK; he was a seafaring man, 21 years of age, and in perfect health. Both his eyes were free from complaint till about the 11th of April, 1793, at which time he was on a voyage home from the East Indies, a sudden mist or dimness appeared before his right eye; this increased very rapidly, and on the 18th of the same month the sight was entirely obscured. The crystalline humour was extracted on the 25th of November; and 27 days after the operation the eye was so far recovered as to admit of the following observations and experiments being made upon it.

In this man we had all the circumstances combined, which seemed to be required to determine how far the crystalline lens was the principal agent in adjusting the eye. The man himself was in health, young, intelligent, and his left eye perfect; the other had been an uncommonly short time in a diseased state, and appeared to be free from every other defect but the loss of the crystalline lens. He very willingly allowed me to make the following experiments on him; and remained in town,

although inconvenient to himself, till they were completed; the greater part of them were instituted by Mr. RAMSDEN, and all of them carried through under his direction.

The experiments were begun on the 22d of December, 1793, at which time the following observations were made upon the imperfect eye. The eye bore the light of the day very well; but was fatigued by strong sunshine, or the glare of candle-light. In weak lights objects were not seen at all by the imperfect eye, but in strong lights they presented a faint image, which appeared at the same distance with that seen by the perfect eye, and close to it, or nearly so, but always to the left.

The imperfect eye, unassisted by glasses, could see objects, but it was with a degree of indistinctness; and this indistinct vision only took place at a distance between six and nine inches. With a double convex glass, the radius of one surface an inch and an half, of the other six inches, the flat side towards the eye, having a focus of $2\frac{1}{4}$ inches, objects appeared most distinct at $4\frac{1}{2}$ inches, and the extremes were $2\frac{1}{2}$ inches, and $5\frac{1}{2}$ inches. The different distances were ascertained by placing one end of a foot rule against the man's forehead, and giving him the book in his own hand, desiring him to carry it to the distance at which he saw best, and afterwards to the two extremes of distinct vision, the upper end of the book being always in contact with the rule; so that the moment he adjusted the book, the distance was read off from the scale. The accuracy with which he brought it to the same point in repeating the experiments, proved his eye to be uncommonly correct; for as he did not himself see the scale, there could be no source of fallacy.

Making these experiments fatigued the eye considerably, and repeating them after very short intervals made the eye water, and gave a slight degree of pain; this, however, soon went off.

In looking at objects through this glass, the image was free from any tinge of colour, unless he directed his eye towards the circumference of the glass, and then it had a considerable tinge, which evidently arose from the prismatic figure of that part of the glass.

A comparative experiment was made upon the perfect eye, with a glass of 15 inches focus. Objects were found in one experiment to appear most distinct at $8\frac{1}{2}$ inches, the extremes 3 inches and 11 inches; in another, most distinct at 7 inches, the extremes as before, 3 and 11 inches.

On the 29th of December, 34 days after the operation, the following experiments were made by candle-light, about six o'clock in the evening.

The experiment with the double convex glass was repeated, the aperture being diminished to $\frac{3}{20}$ of an inch; objects appeared most distinct at 5 inches, the extremes 3 inches and $7\frac{3}{4}$ inches. The aperture was diminished to $\frac{3}{40}$ of an inch, and vision appeared most distinct at 5 inches, the extremes $3\frac{1}{2}$ inches and 7 inches. When the aperture was reduced to $\frac{1}{20}$ of an inch, the inflexion of the rays produced the appearance of a speck, which obscured his vision.

By diminishing the aperture, spherical aberration was in a great measure corrected, and vision rendered more distinct.

A plano-convex glass of $2\frac{7}{8}$ inches focus, with the plane towards the eye, was now applied, and the objects were most distinct at 6 inches, but by no means well defined: the aper-

ture was now reduced to $\frac{4}{30}$ of an inch, and objects appeared much more distinct at $5\frac{1}{2}$ inches; when the glass was brought within half an inch of the eye, objects were still more distinct, and were seen at 5 inches.

The eye was less affected by these than the former experiments, nor was it fatigued by the light of the candle. In strong lights a faint image was seen by the imperfect eye, and always to the left of the other.

The perfect eye, with a glass of 15 inches focus, saw objects most distinctly at $8\frac{1}{2}$ inches, the extremes $3\frac{1}{2}$ inches and $11\frac{1}{4}$ inches.

As these experiments were made with a view to determine whether the eye, when deprived of its crystalline humour, had a power of adjusting itself to different distances; that being ascertained, they were not prosecuted further, on account of the tender state of the man's eye, who went into the country as soon as they were completed.

On the 4th of November, 1794, the man returned to London, and submitted himself to be the subject of further experiments. This afforded us an opportunity of ascertaining the comparative adjustment of the two eyes, when by means of different glasses they were brought to see distinctly at nearly the same focal distance: an experiment we had been unable to make before for want of proper glasses.

Sir HENRY ENGLEFIELD, who will be found to have given us his assistance in the subsequent part of this investigation, was present at this experiment, and was much astonished, as we had been in the former ones, at the accuracy with which the man's eye was adjusted to the same distance in the repeated trials that were made with it.

The perfect eye, with a glass of $6\frac{1}{2}$ inches focus, had distinct vision at 3 inches; the near limit was $1\frac{7}{8}$ inch, the distant one less than 7 inches.

The imperfect eye, with a glass $2\frac{\text{[illegible]}}{10}$ inches focus, with an aperture $\frac{3}{40}$ of an inch, had distinct vision at $2\frac{7}{8}$ inches, the near limit $1\frac{7}{8}$ inch, the distant one 7 inches.

From the result of this experiment we find that the range of adjustment of the imperfect eye, when the two eyes were made to see at nearly the same focal distance, exceeded that of the perfect eye.

These experiments were made by Mr. RAMSDEN, who took particular care to avoid every thing that might be productive of error or deception; and repeated them several times before any conclusions were drawn from them. Several others were made on the same subject, but as they only tended to confirm those already mentioned, it would be taking up the time of this learned Society unnecessarily to detail them.

It may be proper to mention a reason which suggested itself to Mr. RAMSDEN, why the point of distinct vision of the imperfect eye appeared to the man himself nearer than it was in reality; it arose from his judging of distinctness by the legibility of the letters, which were easier read when they subtended a greater angle (from the imperfection of his eye) than at his real point of distinct vision.

The result of these experiments convinced us that the internal power of the eye, by which it is adjusted to see at different distances, does not reside in the crystalline lens; we were also satisfied by the facts and arguments adduced in Mr. HUNTER's letter on this subject, published in the first part of the last volume of the Philosophical Transactions, that it

does not arise from a change in the general form of the globe of the eye; we therefore abandoned both of these theories.

It suggested itself that any change in the curve of the cornea (could it be produced), would vary the refraction of the rays, so as considerably to alter the focus of the eye; and upon considering this subject, Mr. RAMSDEN made a rough calculation, from which it appeared, that a very small alteration in that part would vary the adjustment of the eye from parallel rays to its shortest distance of distinct vision.

This opened to us a new field of inquiry, and I endeavoured to ascertain how far the cornea admitted of such a change, and if it did, how far that change operated in producing this particular effect.

For the first of these purposes I made the following experiments in the presence of Mr. RAMSDEN.

A portion of the cornea $\frac{1}{8}$ of an inch broad, and $\frac{11}{20}$ of an inch long, was removed from the eye of a person 40 years of age, two days after death, with a part of the sclerotic coat on each side attached to it. This was laid upon a piece of glass immersed in water, under which was a scale divided into very minute parts, these divisions being very readily seen through the glass. One end of the cornea was made fast by fixing the sclerotic coat, and a force was applied to the other; this power was found capable of elongating the cornea $\frac{1}{20}$ part of an inch; and on removing it, the cornea recovered itself to its original length. In different trials it varied in the quantity of elongation, but in all of them it was fully $\frac{1}{11}$ part of the whole length, or diameter of the cornea.

The elasticity of the cornea being thus ascertained, encouraged me to proceed in the anatomical investigation; and

I was desirous of determining more exactly than had hitherto been done, the precise insertion of the tendons of the four straight muscles of the eye, so as to know whether their action could be extended to the cornea or not.

In dissecting these muscles to their termination, I found that they approached within $\frac{1}{8}$ of an inch of the cornea, before their tendons became attached to the sclerotic coat upon which they lay; it was evident that they did not terminate at this part, but were so united as to be difficultly separated by dissection; I therefore endeavoured by gentle force to pull them asunder, as in that way the parts would separate in the direction of their fibres. In doing this, they not only admitted of separation to the edge of the cornea, but brought away a lamina of the cornea with them. I thought this would be better seen in an eye after putrefaction had begun to take place, but found that in that state it could scarcely be demonstrated; while in the recent eye the whole of the external lamina of the cornea could be brought away along with the four straight muscles, leaving the surface underneath uniform, but without polish, and upon the same plane with the sclerotic coat, of which it was a continuation.

As this was a new fact, and a very important one, shewing a connection between these muscles and the cornea, I have dried the parts, and preserved them in that state, to shew the mode in which the tendons of the straight muscles are lost in the cornea, giving it the appearance of a central tendon.

The cornea from this investigation is proved to be composed of two laminæ, the external a continuation of the tendons of the four straight muscles, the other a continuation of the sclerotic coat, and the uniting medium between them is not unlike very fine cellular membrane.

If the cornea is examined at its attachment to the sclerotic coat and tendons of the straight muscles, it appears to be of exactly the same thickness with those parts, but grows thicker towards the centre; this increase of thickness is principally in the external lamina, for when that is removed, the other appears equally so through its whole extent.

To ascertain that the cornea is really thickest in the middle, I made a transverse section of it, and Mr. Ramsden, with several other gentlemen, examined the cut edge through a magnifying glass, and all of them were satisfied with the fact of the central part being evidently thicker than that which was nearer to the circumference.

It is necessary to mention, that in stretching the cornea the central part yields most readily to the power applied; this is so much the case, that if the cut edge of the cornea is examined while it is several times drawn out and allowed to contract again, the change in the centre will be found the most distinct; the principal elasticity appearing to reside in that part.

Before these experiments were made upon the cornea, Mr. Ramsden had promised me that he would contrive an instrument by which the cornea might be examined, while the eye was adapting itself to different distances; so as to enable us to decide whether any change took place at these times in its external figure.

When I state to this learned Society, that seven months elapsed before the apparatus for this experiment was completed, they will not attribute it to a want of solicitude on my part, or a want of attention in Mr. Ramsden; but to delays which must necessarily occur to an artist so extensively employed in business, and at the same time so ready to engage

both from inclination, and the urgent requests of his friends, in promoting philosophical inquiries.

On the 31st of July, 1794, we were enabled to begin our experiments, for which the following apparatus was constructed.

A thick board was fixed to a strong upright support, directly opposite to the window of Mr. RAMSDEN's front room on the first floor, which looks up Sackville-street, at the distance of one foot from the window. In this board was a square hole, large enough to admit a person's face, the forehead and chin resting against the upper and lower bars, and the cheek against either of the sides, so that when the face was protruded, the head was steadily fixed by resting on three sides, and in this position the left eye projected beyond the outer surface of the board.

On the outside of the board, or that next the window, upon the left of the square hole, was fixed a microscope, so placed as to take into its field the lateral part of the front of the cornea, which projects beyond the eyelids. The microscope had not only a movement directly forwards, but by means of endless screws, had also a vertical and horizontal motion, without which the experiments could not have been made with any degree of precision.

From the upper part of the square hole an horizontal brass beam projected towards the window, with joints, by which it could be lengthened or shortened, and at the end of this a brass plate was suspended, which admitted of being raised or depressed, so as to bring a small hole that had been drilled through it directly opposite to the eye.

With this apparatus we began our experiments; and I consider it as a fortunate circumstance that Sir HENRY ENGLEFIELD

arrived in town the night before they were made; he very cheerfully gave us his assistance the moment I made the request.

Sir HENRY, from his practical knowledge of mathematical instruments, and the habit of making observations with them, rendered us very material assistance in the course of our experiments, and I feel myself obliged to him for remaining in town till they were completed. To Mr. RAMSDEN and myself it was a particular satisfaction to have an evidence who had no presupposed opinion, therefore impartial; whose knowledge of the subject enabled him to form a judgment of the results, and to correct any error we might fall into in conducting the experiments. This circumstance will also give to the experiments an additional claim upon the notice of this learned Society.

The first experiment was made at three o'clock, at which were present Sir HENRY ENGLEFIELD, Mr. RAMSDEN, and myself. It required some time, and considerable ability, in which I can claim no part, to adjust the microscope, and bring the cornea into its field; when this was done, the appearances were so different from what were expected, that we had a difficulty in recognizing the object; all that could be seen was 4 curved lines, but even these were rendered confused by reflections from the cross bars of the sash of the window. Upon throwing up the sash, the curved lines became very distinct, and that which appeared the inner one in the microscope, was ascertained to be the convex projecting surface of the cornea.

This being determined, the person whose eye was the object of the experiment was desired to look at the corner of a chimney at the upper end of Sackville-street, a distance of 235

yards, through the hole in the brass plate, and afterwards to look at the edge of the small hole itself, which was only 6 inches from the eye. In doing this several times, the curved lines were seen to separate from each other; and the microscope required being withdrawn from the object whenever the person's eye was adjusted to the near distance; but the very reverse took place when it was fixed on the distant one.

In making these experiments, the least motion of the head carried the cornea out of the field of the microscope; it was therefore necessary that the two objects should be exactly in the same line respecting the eye, and that the person should remain silent. When he complied with any request which had been made, he signified by touching the knee of the observer with his hand, that he had done so. This experiment was made upon the eyes of all present, and the same appearances were uniformly observed; and after several trials we became so familiar with the appearances, that the observer only required information of the adjustment having been changed, to enable him to tell which of the objects the eye was fixed upon.

August the 1st, about four o'clock, these experiments were repeated, and after several attempts were made, without success, to explain the cause of the curved lines, we found it necessary to shade a part of the window, to take off the glare of light which fatigued the eye, and rendered it unsteady; this made the curved lines less distinct; and when the whole window was shaded they disappeared altogether, leaving a very distinct view of the whole thickness of the cornea, with a well defined line formed by its anterior projecting surface. This discovery proved the curved lines to be reflections from the sides of the window upon the cornea; but as it was not made

till six o'clock, we were obliged to postpone any further observations upon it.

August the 3d, at seven o'clock in the morning, Mr. RAMSDEN and myself resumed our experiments, Sir HENRY ENGLEFIELD being unable to attend at that hour. The eye of the person under observation was shaded from the light by shutting the half of the window-shutter directly before it, and to direct the sight to pass through it, a hole was bored in the shutter; the other half of the shutter was turned back, so as to take off the side light, only letting in enough to illuminate the cornea; in this state the cornea was very distinctly seen, and the former experiments were repeated upon it, with a micrometer wire in the focus of the eye-glass, so placed as accurately to oppose the anterior edge of the cornea.

The motion of the cornea became now perfectly distinct; its surface remained in a line with the wire when the eye was adjusted to the distant object, but projected considerably beyond it when adapted to the near one; and the space through which it moved was so great as readily to be measured by magnifying the divisions upon a scale, and comparing them; in this way we estimated it at the 800 part of an inch, a space distinctly seen in a microscope magnifying 30 times.

It may not be improper, for the sake of accuracy, to mention that the hole made in the window-shutter did not admit of seeing up Sackville-street, so that the distant object was now only at 90 feet, which is rather less than is necessary for parallel rays; a circumstance, so far as it can be considered, in favour of the experiment, as a more distant object must have increased the effect upon the cornea. Having satisfied ourselves fully respecting the result of this experiment, we desisted from further trials.

At twelve o'clock of the same day, we prevailed on Sir HENRY ENGLEFIELD to make the experiment on my eye, without giving him any information of the observations that had been made in the morning. He was very much struck with the distinctness of the cornea; and told me without difficulty the different objects to which my eye was adjusted, and was as fully satisfied as either Mr. RAMSDEN or myself with the result of the experiment.

Mr. RAMSDEN now made the same experiment on Sir HENRY's eye, but was unable to retain it in the field of the microscope; the motion of the cornea was always in one direction, and very irregular; after repeated trials, equally unsatisfactory, the eye became so fatigued that he was obliged to desist.

August the 4th, Mr. RAMSDEN repeated the experiment on Sir HENRY's eye, to ascertain if possible the cause of his former want of success, and found the same circumstances again take place; the curve of the cornea moved always in the same direction, never returning to the wire. This could not be accounted for, till it was accidentally discovered to arise from the motion of his hand in touching the knee of the observer, for when that was omitted, the experiment was followed by the same results as those made on the rest of the company. I have been more particular in mentioning this circumstance, as it shows that the most trifling things may interfere with the result of the experiment, and that it required a considerable degree of nicety and management in adjusting the instrument, without which the experiment could not have been made.

August the 28th, the former experiments were repeated by Sir HENRY ENGLEFIELD, Mr. RAMSDEN, and myself, upon the eye of a young lad, and the result was similar to the others, the

motion of the cornea was uncommonly distinct. Sir HENRY now became the subject of the experiment, and changed the adjustment of his eye from one distance to another in a very irregular manner, without giving the smallest information, with a view to embarrass Mr. RAMSDEN who was the observer, but without effect, for Mr. RAMSDEN was able to tell every change in distance he had made, without a single mistake; this exceeded our expectation, and appeared to us so satisfactory that we required no further proofs of the truth of our former observations.

Before we concluded our experiments, every mode that could be devised was put in practice to see how far there might be any deception; the eye was moved upon its axis, and in different directions, but these motions did not give at all similar appearances to those seen in the adjusting of the eye to different distances.

From the different experiments which I have had the honour to lay before this learned Society, I shall consider the following facts to have been ascertained.

1st, That the eye has a power of adjusting itself to different distances when deprived of the crystalline lens; and therefore the fibrous and laminated structure of that lens is not intended to alter its form, but to prevent reflections in the passage of the rays through the surfaces of media of different densities, and to correct spherical aberration.

2d, That the cornea is made up of laminæ; that it is elastic, and when stretched, is capable of being elongated $\frac{1}{11}$ part of its diameter, contracting to its former length immediately upon being left to itself.

3d, That the tendons of the four straight muscles of the eye

are continued on to the edge of the cornea, and terminate, or are inserted, in its external lamina; their action will therefore extend to the edge of the cornea.

4th, That in changing the focus of the eye from seeing with parallel rays to a near distance, there is a visible alteration produced in the figure of the cornea, rendering it more convex; and when the eye is again adapted to parallel rays, the alteration by which the cornea is brought back to its former state is equally visible.

Having supported these facts by the evidence of anatomical structure, and absolute demonstration, I shall consider them to be established; and make some observations upon the muscular and elastic power by which so very curious an effect as the adjustment of the eye is produced.

The four straight muscles of the eye are attached to the bottom of the bony orbit near the foramen opticum; they become broader as they pass forward, and when arrived at the anterior part of the eye-ball, are insensibly changed for tendons; these adhere to the sclerotic coat, and terminate in the external lamina of the cornea, which appears to be a continuation of them.

When we consider the situation of these muscles, it is evident that their action will produce three very different effects upon the eye, according to circumstances. When they act separately, they will move the eye in different directions; when together, with only a small quantity of contraction, they will steady the eye-ball; and when this is increased they will compress the lateral and posterior parts of the eye. This compression of the eye will force the aqueous humour forwards against the centre of the cornea, while the circumference is steadied

by the muscles, so that the radius of curvature of the cornea will be rendered shorter, and its distance from the retina increased.

That the eye-ball cannot be made to recede in the orbit by any of these actions, is sufficiently proved by its not having done so in any of the experiments.

These muscles are uncommonly large, and come much further forward than appears necessary for the purposes generally assigned to them; but when applied to so important an office as that we have just stated, their size, and anterior insertion, are easily explained.

It may be imagined that I have allotted to these muscles a greater variety of uses than is compatible with the simplicity of the general laws of the animal œconomy; but to prove this not to be the case, I shall only bring the biceps flexor cubiti as an instance of a similar kind. That muscle is attached to the scapula by both its heads, one of which passes through the joint of the shoulder, they afterwards unite, and their common tendon is inserted into the radius; when the muscle contracts, the first effect will be to steady the joint of the shoulder; if the contraction is increased, it will rotate the radius, and if still more increased, bend the fore-arm.

There are many instances in animal bodies of elasticity being substituted for muscular action, but this in the eye is by much the most beautiful of those applications.

In the vascular system the arteries are composed of muscular fibres, and an elastic substance; in the natural easy state of the circulation, the re-action in the larger vessels is principally the effect of elasticity, but when increased, it is the effect of muscular contraction.

The claws of the lion are drawn up, and supported from

the ground, by means of elastic ligaments; but they are brought down for use, which is an action not so often required, by muscles.

In the adjustment of the eye it is the same; the state fitted for parallel rays is the effect of elasticity, but that for nearer distances, which is less frequently wanted, is the effect of muscular action.

In these different instances, the intention is uniformly to avoid the expence of muscular action whenever the effect can be produced in any other way, as muscular actions consume a considerable quantity of blood, which is the nourishment of the body.

That the adjusting the eye to near distances is the effect of an action, or exertion, was very evident to every gentleman concerned in these experiments. In changing the focus of our eyes, we were much astonished, particularly Sir HENRY ENGLEFIELD, at the exertion required to adjust the eye to the near distances, and the facility with which it was adapted to distant ones; the first was a strain upon the eye, the second appeared a relief to it.

When the eye was intent upon the near object, it required the attention to be constantly kept up, or the object became indistinct; and if we looked at it beyond a certain time, the eye was so much fatigued as to lose it at intervals. This corresponds with other muscular actions, for whenever muscles are kept long in one state they begin to vibrate involuntarily.

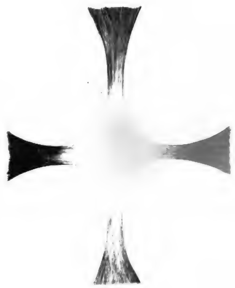
These circumstances explain what may be called a *coup d'œil,* or the distinctness with which an object is seen when the eye is first fixed upon it. This arises from the nice adjustment produced by the muscles when first thrown into

action, which they cannot keep up, being unable to remain long in the same state; nor can they, after having been used for any time, return to this adjustment with the same exactness.

The change that takes place in the eye at an advanced period of life, by which it loses its adjustment to very near, and very distant objects, does not arise from any defect in the muscles, as might at first be imagined, since that would not account for the eye being unable to see with parallel rays; nor is there any obvious reason why these muscles should lose their powers, while others, which are not apparently so strong, if we may judge by their effects, retain their full action long after the eye has undergone this change.

This defect in the eye I am led to believe is brought on by the cornea losing its elasticity as we advance in life, neither contracting nor being elongated to its usual extent, but remaining in a middle state. That elastic substances in the body do undergo such a change may be well illustrated in the vascular system. The aorta is composed almost entirely of elastic substance, and there is probably no part of the body, at an advanced age, which is so often found to have lost its natural action; it appears to undergo a change from age alone, becoming inelastic, and then taking on diseases of different kinds, as being ossified, or becoming aneurismal; but in neither of these diseases is it found to be contracted, although often the reverse, and when disease has not supervened, the artery more commonly remains in the middle state.

The cornea having similar properties must be liable to a similar change, but its action being less constant, and the power which it is to resist being weaker, the change will be probably more gradual and less in degree, but sufficient to



account for the alteration we find in the focus of the eyes of old people.

There are many other circumstances respecting vision, and many which occur in disease, that may be explained by a knowledge of these facts; but as this lecture is only intended to establish the facts themselves, in doing which I have already taken up too much of the time of this learned Society, I shall at some future period consider their application to the phænomena of vision in health, and disease.

EXPLANATION OF THE PLATE. (Tab. I.)

Portions of the four straight muscles of the eye, with their tendons insensibly lost in the external lamina of the cornea, stretched out and dried. The tendons become broader as they approach the cornea, and form a circle of which the cornea appears to be a continuation.

II. *The* Bakerian *Lecture. Observations on the Theory of the Motion and Resistance of Fluids; with a Description of the Construction of Experiments, in order to obtain some fundamental Principles. By the Rev.* Samuel Vince, *A. M. F. R. S.*

Read November 27, 1794.

However satisfactory the general principles of motion may be, when applied to the action of bodies upon each other, in all those circumstances which are usually included in that branch of natural philosophy called MECHANICS, yet the application of the same principles in the investigation of the motions of FLUIDS, and their actions upon other bodies, is subject to great uncertainty. That the different kinds of airs are constituted of particles endued with repulsive powers, is manifest from their expansion when the force with which they are compressed is removed. The particles being kept at a distance by their mutual repulsion, it is easy to conceive that they may move very freely amongst each other, and that this motion may take place in all directions, each particle exerting its repulsive power equally on all sides. Thus far we are acquainted with the constitution of these fluids; but with what absolute degree of facility the particles move, and how this may be affected under different degrees of compression, are circumstances of which we are totally ignorant. In respect to those fluids which are denominated liquids, we are still less acquainted

with their nature. If we suppose their particles to be in contact, it is extremely difficult to conceive how they can move amongst each other with such extreme facility, and produce effects in directions opposite to the impressed force without any sensible loss of motion. To account for this, the particles are supposed to be perfectly smooth and spherical. If we were to admit this supposition, it would yet remain to be proved how this would solve all the phænomena, for it is by no means self-evident that it would. If the particles be not in contact, they must be kept at a distance by some repulsive power. But it is manifest that these particles attract each other, from the drops of all perfect liquids affecting to form themselves into spheres We must therefore admit in this case both powers, and that where one power ends the other begins, agreeable to Sir ISAAC NEWTON's * idea of what takes place not only in respect to the constituent particles of bodies, but to the bodies themselves. The incompressibility of liquids (for I know no decisive experiments which have proved them to be compressible) seems most to favour the former supposition, unless we admit, in the latter hypothesis, that the repulsive force is greater than any human power which can be applied. The expansion of water by heat, and the possibility of actually converting it into two permanently elastic fluids, according to some late experiments, seem to prove that a repulsive power exists between the particles; for it is hard to conceive that heat can actually create any such new powers, or that it can of itself produce any such effects. All these uncertainties respecting the constitution of fluids must render the conclusions deduced from any *theory* subject to considerable

* See his Optics, Que. 31.

errors, except *that* which is founded upon such experiments as include in them the consequences of all those principles which are liable to any degree of uncertainty.

A fluid being composed of an indefinite number of corpuscles, we must consider its action, either as the joint action of all the corpuscles, estimated as so many distinct bodies, or we must consider the action of the whole as a mass, or as one body. In the former case, the motion of the particles being subject to no regularity, or at least to none that can be discovered by any experiments, it is impossible from this consideration to compute the effects; for no calculation of effects can be applied when produced by causes which are subject to no law. And in the latter case, the effects of the action of one body upon another differ so much, in many respects, from what would be its action as a solid body, that a computation of its effects can by no means be deduced from the same principles. In mechanics no equilibrium can take place between two bodies of different weights, unless the lighter acts at some mechanical advantage; but in hydrostatics, a very small weight of fluid may, without its acting at any mechanical advantage whatever, be made to balance a weight of any magnitude. In mechanics, bodies act only in the direction of gravity; but the property which fluids have of acting equally in all directions, produces effects of such an extraordinary nature as to surpass the power of investigation. The indefinitely small corpuscles of which a fluid is composed, probably possess the same powers, and would be subject to the same laws of motion, as bodies of finite magnitude, could any two of them act upon each other by contact; but this is a circumstance which certainly never takes place in any of the aerial fluids, and probably not in any

liquids. Under the circumstances, therefore, of an indefinite number of bodies acting upon each other by repulsive powers, or by absolute contact under the uncertainty of the friction which may take place, and of what variation of effects may be produced under different degrees of compression, it is no wonder that our theory and experiments should be so often found to disagree.

Sir ISAAC NEWTON seems to have been well aware of all these difficulties, and therefore in his PRINCIPIA he has deduced his laws of resistance, and the principles upon which the times of emptying vessels are founded, entirely from experiment. He was too cautious to trust to theory alone, under all the uncertainties to which he appears to have been sensible it must be subject. He had, in a preceding part of that great work, deduced the general principles of motion, and applied them to the solution of problems which had never before been attempted; but when he came to treat of fluids, he saw it was necessary to establish his principles upon experiments; principles, not indeed mathematically true, like his general principles of motion before delivered, but, under certain limitations, sufficiently accurate for all practical purposes.

The principle to be established in order to determine the time of emptying a vessel through an orifice at the bottom, is the relation between the velocity of the fluid at the orifice and the altitude of the fluid above it. Most writers upon this subject have considered the column of fluid over the orifice as the expelling force, and from thence some have deduced the velocity at the orifice to be that which a body would acquire in falling down the *whole* depth of the fluid; and others that acquired in falling through *half* the depth, without any regard

to the magnitude of the orifice; whereas it is manifest from experiment, that the velocity at the orifice, the depth of the fluid being the same, depends upon the proportion which the magnitude of the orifice bears to the magnitude of the bottom of the vessel, supposing, for instance, the vessel to be a cylinder standing on its base; and in all cases the velocity, *cæteris paribus*, will depend upon the ratio between the magnitude of the orifice and that of the surface of the fluid. Conclusions thus contrary to matter of fact show, either that the principle assumed is not true, or that the deductions from it are not applicable to the present case. The most celebrated theories upon this subject are those of D. BERNOUILLI and M. D'ALEMBERT; the *former* deduced his conclusions from the principle of the *conservatio virium vivarum*, or as he calls it, the *equalitas inter descensum actualem ascensumque potentialem*, where by the *descensus actualis* he means the actual descent of the centre of gravity, and by the *ascensus potentialis* he means the ascent of the centre of gravity, if the fluid which flows out could have its motion directed upwards; and the *latter* from the principle of the *equilibrium* of the fluid. This principle of M. D'ALEMBERT leads immediately to that assumed by D. BERNOUILLI, and consequently they both deduce the same fluxional equation, the fluent of which expresses the relation between the velocity of the fluid at the orifice, and the perpendicular altitude of the fluid above it. How far the principles here assumed can be applied in our reasoning upon fluids, can only be determined by comparing the conclusions deduced from them with experiments.

The fluxional equation above mentioned cannot in general be integrated, and therefore the relation between the velocity

of the fluid at the orifice and its depth cannot from thence be determined in all cases. If the magnitude of the orifice be indefinitely less than that of the surface of the fluid, the equation gives the velocity of the effluent fluid to be equal to that which a body would acquire by falling *in vacuo* through a space equal to the depth of the fluid. But the velocity here determined is not that at the orifice, but at a small distance from the orifice; for the fluid flowing to the orifice contracts the stream, and the velocity being inversely as the area of the section, the velocity continues to increase as long as the stream, by the expelling force of the fluid, keeps diminishing, and when the stream ceases to be contracted by that force, at that section of the stream called the *vena contracta*, the velocity is that which a body would acquire in falling through a space equal to the depth of the fluid. If, therefore AB *cd* EF (Tab. II. fig. 1.) be the vessel, *cd* the orifice, *c m n d* the form of the stream till it comes to the *vena contracta*, then this investigation supposes AB *c m n d* EF to be the form of the vessel, and *m n* the orifice, the fluid flowing through *c m n d* just as if the vessel were so continued. But as the proposition is to find the velocity of the fluid going out of the vessel, it may perhaps appear an arbitrary assumption to substitute the orifice *m n* instead of *c d*, when no such a quantity as *m n* appears in the investigation. If, however, we grant that the expelling force must act without any diminution until the fluid comes to *m n*, it seems that from the principles here assumed we ought to substitute *m n* instead of *c d*, as otherwise we get the velocity generated by the action of only a part of the force. The conclusion here deduced agrees very well with experiment; but an application of the same principles to another case differs so

widely from matter of fact, as to render it very doubtful how far the principles here applied can be admitted. And if we were to grant the application of the principles here assumed, so far as regards the determination of the velocity, yet the time of emptying a vessel can by no means be deduced from it.

In order to determine the time of emptying a vessel, we must know both the area of the orifice $c\,d$, and the velocity at that orifice. Now the theory gives only the velocity at $m\,n$; and as it gives not the ratio of $m\,n$ to $c\,d$, the velocity at the orifice cannot be deduced from thence, and therefore we cannot find the time of emptying. No theory whatever has attempted to investigate the ratio of mn to cd; it is well known that that is only to be determined by an actual mensuration. When the orifice is very small, Sir ISAAC NEWTON found the ratio to be that of 1 to $\sqrt{2}$; when the orifice is larger, the ratio approaches nearer to that of equality. We cannot therefore, even in the most simple case, determine, by theory alone, the time in which a vessel will empty itself.

If A B C D (fig. 2.) be a vessel filled with a fluid, and a pipe $m\,n\,r\,s$ be inserted at the bottom, $m\,n$ being very small in respect to B C, then, according to the theory of D. BERNOUILLI, the fluid ought to flow out of the pipe at $r\,s$ with the same velocity it would out of a vessel A L M D through the orifice $r\,s$. Now in this latter case, the velocity, according to his own principles, varies as the square root of L A, and therefore it varies in the same ratio in the former case; hence if the length $m\,r$ of the pipe bears but a very small proportion to A B, the velocity with which the fluid flows out of the pipe will be very nearly equal to the velocity with which it would

flow through an orifice at the bottom equal to $r s$ or $m n$, the pipe being supposed to be cylindrical. To find how far this conclusion agrees with experiment, I made a cylinder 12 inches deep, and at the bottom I made a small circular orifice, whose area was about the 130th part of the area of the bottom of the cylinder: I also put a cylindrical pipe into the bottom, whose internal diameter was exactly equal to that of the hole, and length 1 inch. Hence, according to the theory, the velocity of the fluid out of the pipe ought to be to the velocity out of the orifice as $\sqrt{13} : \sqrt{12}$, or as 26 : 25 nearly. But by experiment, the quantity of fluid which run through the pipe in 12″ (the vessel being kept full) was to the quantity which run through the orifice in the same time, very nearly in the ratio of 4 to 3, and consequently that ratio expresses the ratio of the velocities; a consequence totally different from that which the theory gives. I then took a vessel of a different base, but the same altitude, and altered the diameter of the orifice and pipe, still keeping them equal, and made the pipe only half an inch long; in this case the velocities, by the theory, ought to have been in the ratio of $\sqrt{12,5}$ to $\sqrt{12}$, or as 49 to 48 nearly; whereas by experiment the ratio of the velocities came out the same as before, that is, as 4 to 3 nearly. I then reduced the pipe to the length of a quarter of an inch, and in that case the velocity did not sensibly differ from that through the orifice. Upon examining the stream, in consequence of this great difference in the two cases, when the lengths of the pipes differed by so small a quantity, I found that in the latter case the stream did not fill the pipe, as it did in the former case, but that the fluid was contracted as when it run through the simple orifice. At what length of pipe the stream will cease

to fill it, is a circumstance to which no theory has ever been applied, but the determination thereof must be a matter of experiment entirely.

I next inserted pipes of different lengths, and found that when the length of the pipe was equal to the depth of the vessel, the velocity of the effluent fluid by theory was to that by experiment as about 7 to 6; and by increasing the length of the pipe, the ratio approached nearer to that of equality. In long pipes, therefore, the difference between theory and experiment is not greater than what might be expected from the friction of the pipes, and other circumstances which may be supposed to retard the velocity.

If the pipe be conical, increasing downwards, the velocity, by theory, is still the same, and consequently the quantity run out will be in proportion to the magnitude of rs. As long as the expelling force can keep the tube full, this appears to be the case; but by increasing the orifice rs, the pipe will, at a certain magnitude, cease to be kept full; at what time this happens must depend entirely upon experiment. But if the pipe decrease, having its orifice rs equal to that of a cylindrical pipe of the same length, the velocity through the former appears, from the experiment I made, to be greater than through the latter in the ratio of 14 to 11.

If the pipe mr (fig. 3.) be inserted horizontally into the side of a vessel, the velocity at the orifice rs, by theory, is always in proportion to the square root of the altitude C D, the orifice being still supposed to be very small compared with the bottom of the vessel. By trying the experiment with pipes of different lengths and of the same diameter, beginning with the shortest and increasing them, it appears that the

velocity first increases and then decreases; and this is a circumstance which has been before observed. If $r s$ be greater than C m, the quantity of fluid which flows out in a given time (the vessel being kept full) appears to be increased in proportion to the increase of $r s$, as long as the expelling force is able to keep the pipe full; but at what magnitude of $r s$ this effect ceases must be determined by experiment. If $r s$ be less than C m, the quantity which flows out is greater than if the pipe were cylindrical, and of the same diameter as $r s$.

The velocities of fluids spouting upwards through an orifice or pipe has not been considered by BERNOUILLI; but the following experiments will show the effects in this case. Let A B C D E F (Tab. II. fig. 4.) be a vessel filled with a fluid, r an orifice, x, y, z, three pipes each an inch long, having their tops on an horizontal line with the orifice; x is cylindrical, of the same diameter as that of the orifice; y is conical, increasing upwards, of the same diameter at the bottom as the orifice; z decreases upwards, of the same diameter at the top as the orifice. In 12″, the quantities which run out through the orifice and pipes x, y, z, (the vessel being kept full) were found to be in the ratio of 7, 9.4, 11.2 and 10.7. Hence the ratio of the velocities through the orifice and pipe x appears to be very nearly in the ratio of 3 to 4, agreeable to what was found to take place for an orifice and short pipe at the bottom. The quantity which run out of the pipe y increased by increasing the diameter at the top, in proportion to that area as nearly as could be ascertained, as long as the expelling force could keep it full; and a greater quantity run out of the pipe z than through the orifice. All this is agreeable to what was found to take place under similar circumstances when the

orifice and pipes were inserted at the bottom. So far therefore as the theory can be applied when the fluid descends perpendicularly, it appears to be applicable also to the case when it spouts upwards.

At the bottom of the vessel A B C D (Tab. II. fig. 5.) having an orifice $r\,s$, I inserted a pipe $a\,x\,y\,z\,w\,v$ conical at the top and cylindrical downwards from it, having the diameter of the cylindrical part equal to that of the orifice, and directly under it. I then stopped the orifice $s\,r$ within, and filled the vessel, and expected, that as there was now no pipe immediately connected with the orifice, the fluid would form the *vena contracta* as if there was no pipe, and that the velocity at the orifice would be the same as through a simple orifice; whereas I found the velocity to be greater, very nearly in the ratio of $\sqrt{2}$ to 1, the length of the pipe being equal to the depth of the cylinder. It appears therefore to flow out with about the same velocity as if the pipe had been continued to the orifice. The fluid therefore must have flowed from the orifice in a cylindrical form, for the pipe was observed to be filled. I see no cause which could prevent the *vena contracta* from being formed. I then stopped the pipe at the bottom $y\,z$, and filled the vessel and pipe, and found the circumstances to be exactly the same.

In order to determine whether there was any pressure of the fluid against the sides of the pipes as it passed through in all their different situations, I pierced some small holes in them at different parts. In the cylindrical pipes, and those in the form of increasing cones, the fluid passed by the holes without being projected out, or without having the least tendency to issue through them; but in the decreasing cones the fluid

spouted out at the holes. In the former cases therefore there was no pressure against the sides of the pipes, but in the latter case there was.

In respect to the motion of the fluid through any of the pipes, I found no difference whether I stopped the pipe at the end of the tube which enters into the vessel, in which case the motion began when the tubes were empty, or whether at the other end, in which case they were full at the commencement of the motion. That the fluid should flow into the top of the pipe faster than it would through an orifice, may probably, in part at least, be owing to the adhesion of the fluid to the pipe, and be thus explained. Although the horizontal motion of the fluid towards the orifice accelerates the velocity after it escapes from the vessel by contracting the stream, yet it must diminish the velocity at the orifice; that is, if the same perpendicular motion were to take place without the horizontal motion, the fluid would flow out faster; for as any motion in a fluid is immediately communicated in every direction, the horizontal motion will produce a motion upwards, and in some degree obstruct the descent of the fluid. If therefore this horizontal motion could be taken away, or any how diminished, the fluid would flow out with a greater velocity. Now if a pipe be fixed, the fluid at the bottom of the vessel flowing towards the orifice will, by its adherence to the vessel, continue to adhere to the sides of the pipe as soon as it arrives there, and by this means almost all the horizontal motion will be destroyed, and converted into a perpendicular motion, for the horizontal motion arises principally from the fluid which flows from and very near to the bottom, where the whole motion is very nearly in that direction. This motion therefore being

thus nearly destroyed, the fluid will be less interrupted at the orifice, and consequently will flow out with a greater velocity. But why the velocity should also be increased either by increasing the length of the pipe, or making it an increasing cone, under certain limitations, is a circumstance which, I confess, I can give no satisfactory reason for.

The abovementioned experiments were made principally with a view to ascertain how far the theory of the motion of fluids can be applied; and the inquiry has led to several circumstances which, I believe, have not been observed before. That the theory is not applicable in all cases is manifest; but that it brings out conclusions in many instances which agree very well with experiment is undoubtedly true. This tends to show, either that the common principles of motion cannot be applied to fluids, and that the agreement is accidental; or that under certain circumstances and restrictions the application is just. Which of these is the case is not, perhaps, easy for the mind to satisfy itself about. Nothing however which is here said is done with any view to detract from the merit of these celebrated authors. They have manifested uncommon penetration, and carried their inquiries upon the subject to an extent, that nothing further can be hoped for or expected; and if they had done nothing else in science, this alone would have ranked them amongst the very first mathematicians. The fault has been *non artificis sed artis*.

Mr. MACLAURIN, in his Treatise on Fluxions, has given a most admirable illustration of the theory of Sir ISAAC NEWTON. It is there a very principal inquiry to determine the ratio of the force which generates the velocity of the descending surface of the fluid to the force of gravity. Now according to that

theory, the pressure on the bottom of the vessel is wholly taken off at the instant of time at which the water begins to flow; and as this conclusion cannot be admitted, we may from hence learn, says the author, that this theory is not to be considered as perfectly exact. It appears therefore to be an important point to determine, what is the pressure of the fluid upon the bottom of a vessel compared with its whole weight at the time the fluid is running out. This may be determined to a great degree of accuracy by experiments constructed in the following manner.

Let A B C D (Tab. III. fig. 6.) be a pair of scales, and O the fulcrum; at the end of the arm C suspend a cylinder E, having an orifice *r s*, immediately under which place a weight *w*, so that the upper surface may be in the *vena contracta*, or at so small a distance below it that gravity can have produced no sensible effect upon the effluent fluid. Stop the orifice *r s*, and fill the cylinder with a fluid, and balance it by a weight W in the other scale. Then open the orifice, and the fluid will run out and strike *w*, and then be caught in the scale D. Now when the orifice is opened and the fluid flows out, the pressure upon the bottom of the cylinder is diminished, part of the fluid now not being supported, notwithstanding which the equilibrium is still continued; which shows that the action of the fluid against *w* is exactly equal to the loss of weight in the cylinder by the motion of the fluid through the orifice. In order therefore to find the diminution of the weight upon the bottom of the cylinder, we have only to find a weight equivalent to the momentum of the fluid against *w*.

Let A B (fig. 7.) be a lever flat on the upper side, suspended by an horizontal axis C D; L a scale hanging from

it, which is to be balanced by a weight W; E is the cylinder suspended to something immoveable at M, having its orifice *r s* as far distant from A B as before it was from the weight in the scale; and let the orifice and scale be equidistant from C D. Stop the orifice, and fill the cylinder; and upon opening the orifice, let one person, by means of a cock at *v* upon a pipe which goes into a reservoir *x y z*, keep the fluid in the cylinder exactly at the same altitude, and another put such a weight *w* into the scale L as shall keep A B exactly in the same position; then the weight *w* is equivalent to the momentum of the fluid against A B, together with the momentum of the fluid entering the top of the cylinder through the pipe. To determine what weight is equivalent to this latter momentum, take away the cylinder E and weight *w*, and bring A B up to the pipe, and let the fluid act upon it, and find what weight *(v)* put into the scale will now keep A B horizontal, and this weight *(v)* will be equivalent to the momentum of the fluid flowing into the cylinder; hence $w - v$ is a weight equivalent to the momentum of the fluid issuing out of the cylinder at the *vena contracta*, and consequently equivalent to the diminution of the pressure upon the bottom after the opening of the orifice. In order to keep the fluid accurately at the same altitude, I should propose to have a floating gage *v* (fig. 8.) with a wire standing perpendicularly upon it, and entering a cylinder *w* attached to the side of the vessel, and of a bore just large enough to give it a free motion; then the cock must be opened and adjusted to give it such an aperture as will keep the top of the wire on a level with the top of the cylinder.

Or we may find the diminution of the pressure upon the bottom on opening the orifice in this manner. In fig. 6, take

away the scale D and balance the cylinder when filled, and let the end C of the beam be made flat at the point from which the vessel is suspended. Then open the orifice of the vessel, having the same provision as before to keep it filled to the same altitude, and place such a weight at C as shall preserve the equilibrium during the time the fluid is in motion, and this weight is equivalent to *w* in the former case. This method is the most simple of the two; but the other includes a circumstance of some consequence, that is, that the momentum of the effluent fluid is exactly equivalent to the weight which the vessel loses. Having thus examined all the circumstances which I proposed respecting the emptying of vessels, I proceed next to the consideration of the doctrine of the resistance of bodies moving in fluids.

When a body moves in a fluid, each particle, in theory, is supposed to act upon it undisturbed by the rest, or the fluid is conceived to act as if each particle, after the stroke, were annihilated, in which case the following particles would exert their force uninterruptedly. This supposition is very far from being true in fact, and accordingly we find very little agreement between theory and experiment. To experiments therefore we must have recourse for any thing satisfactory upon this subject. I therefore constructed the machine which is here described, whereby both the absolute quantity of resistance in all cases may be very accurately determined, and the law of its variation under different degrees of velocity.

A B, C D (Tab. IV. fig. 9.) are two cross pieces of wood firmly connected together, with screws at each end, so that it may be fixed upon any plane; E G F is a frame fixed upon A B; *m n* a small cylindrical well polished iron axis, having

the lower end made conical, and an hollow conical piece to receive it, the upper end passing through G in a polished nut of iron just big enough to give it a free motion; on the top of this axis there are fixed four arms *a*, *b*, *c*, *d*, having each a plane *b*, *g*, *f*, *e*, which may be either of pasteboard or tin, and are thus fixed on. A wire has one end made very flat to which the plane is fixed, and the other end is left round and passes under two small staples made of wire, fixed into the arm so tight that you can but just turn it, so that if you fix the plane in any position it will remain there without any hazard of changing it. Two fine silk lines are wound together round the axis, one leaving the axis on one side and the other on the opposite side, and each, passing over a pulley, is connected to a scale; by this means the lines when drawn by weights put into the scales will give the axis a rotatory motion, and will act in opposite directions, and therefore if equal weights be put into the scales they will destroy each other's effects, so far as regard the position of the axis, so that neither the friction at the bottom nor at the nut at the top will be at all affected by whatever additional weights may be thus added. In respect to any additional friction at the pullies by the increase of weight, that may be diminished so as to become insensible, by increasing the radius of the pullies, and making the ends of their axes conical and letting them turn in a conical orifice, so that they may rest just at their points. If we allow the friction at the axis to be one-fifth of the weight added, which is certainly a great allowance for such an axis well polished, and the radius of the pulley be to the radius of that conical part of the axis where it rests as one hundred to one, then the effect of the friction would be only the five hundredth part of

the whole weight; and even this might be diminished one hundred times more by using friction wheels; but this is a degree of accuracy which, I think, can never be required. We might also diminish the friction at the nut, if required, by letting the axis on those two sides towards which the lines act rest between two friction wheels. If the arms should be very long, it may be necessary to fix an upright piece upon K, and connect the extremity of the sails to the top thereof by a string or wire. When this machine is applied to find the resistance of water, the axis $m n$ must be produced up above K, and the string applied to that part; the machine must be immersed in a large reservoir of water, leaving the part of the axis to which the string is applied above the surface. Before we proceed to the application, we must investigate a point called the centre of resistance.

Def. If a plane body revolve in a resisting medium about an axis by means of a weight acting therefrom, that point into which if the whole plane were collected it would suffer the same resistance, I call the *centre of resistance.*

Let a be the area of the plane, and $\dot{a}$ the fluxion of the area at any variable distance x from the centre of the axis, and d the distance of the centre of resistance from that of the axis. Now the effect of the resistance of $\dot{a}$ to oppose the weight is, from the property of the lever, as the resistance multiplied into its distance from the axis, or as $x\dot{a}$; but the resistance is supposed to vary as the square of the velocity (which is found by experiment to be true under certain limitations), or as the square (x^2) of its distance from the axis; hence the effect of the resistance of $\dot{a}$ to oppose the weight is as $x^3\dot{a}$; therefore the whole effect is as the fluent of $x^3\dot{a}$. For the

same reason the effect of the resistance of the whole plane a at the distance d is as d^3a; hence $d^3 a =$ flu. $x^3 \dot{a}$, consequently $d = \sqrt[3]{\frac{\text{flu.}\, x^3 \dot{a}}{a}}$.

If the plane be a parallelogram, two of whose sides are parallel to the arms, and m and n the least and greatest distances of the other two sides from the axis, then $d = \sqrt[3]{\frac{n^4 - m^4}{4n - 4m}} = \sqrt[3]{\frac{\overline{n^2 + m^2} \times \overline{n + m}}{4}}$.

Now to find the resistance of the planes striking the fluid perpendicularly, first set them parallel to the horizon, so that they may move edge-ways, or in their own plane, and let two equal weights be put, one into each scale, such as to give the arms an uniform velocity, and then these weights together (w) will be just equivalent to the friction of the axis and the resistance of the arms. Then place the planes perpendicular to the horizon by a plumb-line, and put in two more equal weights, one into each scale, making together W, so as to give the planes the same uniform velocity as before. Then, from what has been already observed, there is no additional friction, and therefore this weight W must be equivalent to the resistance of the planes. But this equivalent weight W acts only at the distance of the radius r of the axis from the centre of motion, whereas the resistance is to be considered as acting at the distance d of the centre of resistance from the centre of motion; hence $d : x :: W : \frac{x}{d} \times W$ the weight acting at the distance d, which is equivalent to the resistance acting at the same distance, and consequently it must be equal to the absolute resistance against all the planes. And to find the velocity, let C feet be the circumference described by the centre of

resistance, and let the sails make one revolution in t seconds; then the velocity will be $\frac{c}{t}$ feet in a second.

To find the resistance when the fluid strikes the planes at any angle, set them to that angle, and find the resistance in the very same manner as before. But here we must set two of the opposite planes inclined one way and two the other, so that the fluid may strike the two former on their upper sides, and the two latter on their under sides, but both at the same angle. This caution is necessary in order to prevent any alteration in the pressure, and consequently in the friction upon the axis in the direction thereof; for the fluid striking the planes obliquely, part of the force will be employed in resisting the motion, and part will act perpendicular thereto, or in the direction of the axis, and this latter effect will manifestly be destroyed by the above disposition of the planes, because this force will act *upwards* against two of the planes, and *downwards* against the other two, and being equal, they will destroy each other's effects. The planes may be set to any angle thus: Take a small quadrant divided into degrees; let $m\,n$ (Tab. IV. fig. 10) be the outward inclined edge of the plane; suspend a plumb-line A B so as just to touch it at n, and at n apply the centre of the quadrant, and let the radius passing through 90° coincide with A B, and turn the plane till $n\,m$ coincides with that degree at which you would have the plane strike the fluid, and the plane stands right for that angle.

To find the resistance of a solid, we must have two such solids equal to each other, and put on at the opposite ends of two of the arms, for with one only its centrifugal force will increase the friction against the nut, whereas with two opposite to each other this effect will be destroyed. We must also get

two thin pieces of lead with the edges feathered off, and of the same weight with the two solids. These must first be put upon the opposite arms, and a weight w found as before. Then the leads are to be taken off, and the solids put on in their place, with that side to go foremost whose resistance is required, and then find W as in the case of the planes; and the absolute resistance will be $\frac{x}{2d} \times$ W upon one of the solids.

By this machine we may find the absolute resistance upon the planes in a direction *perpendicular* to that of their motion. For let the lower end of the axis, instead of resting upon the base of the frame, stand upon one end of an horizontal lever, like that in figure the seventh, and let it be balanced by a weight in a scale hanging at the same distance on the other side of the fulcrum, when the sails have acquired an uniform motion, with the planes horizontal, or when moving edge-ways. Then turn the planes to any angle, and add equal weights to the scales R and T, until the planes have acquired the same uniform velocity as before, and put a weight P into the scale at the other end of the lever, which shall now just balance it, and P will be the absolute resistance of the fluid in a direction perpendicular to the motion of the planes.

The law of resistance, when the velocity varies, may be thus found. Let w, as before, be the sum of the two equal weights which will give the planes an uniform horizontal motion when they move edge-ways. Then set them perpendicular to the horizon, and let W be the sum of the two equal weights, put one into each scale, in order to give the sails the same uniform velocity. Take out these two equal weights, and put in two other equal weights, together equal to Q, such as shall give the planes an uniform velocity double to that before given; then the

Fig. 2.

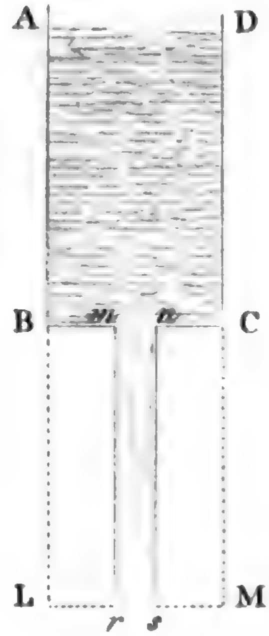
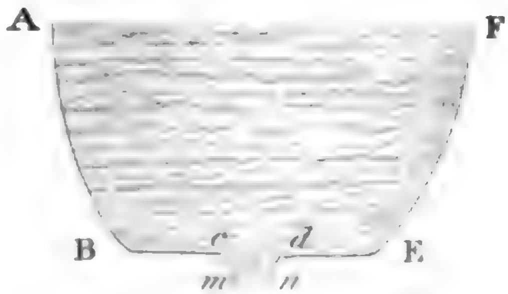


Fig. 1.



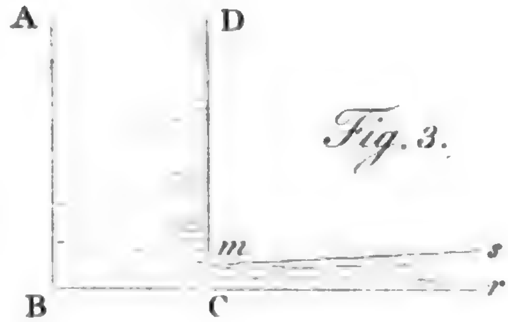


Fig. 3.

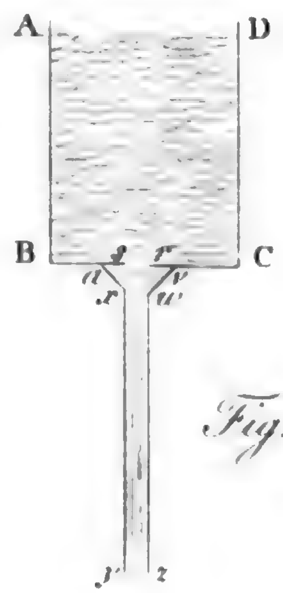


Fig. 5.

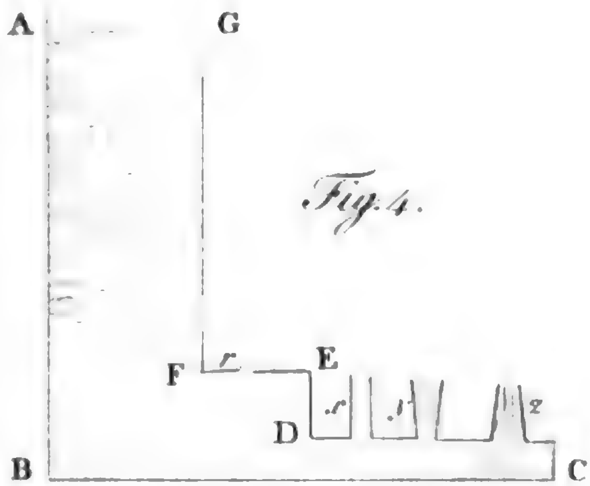


Fig. 4.

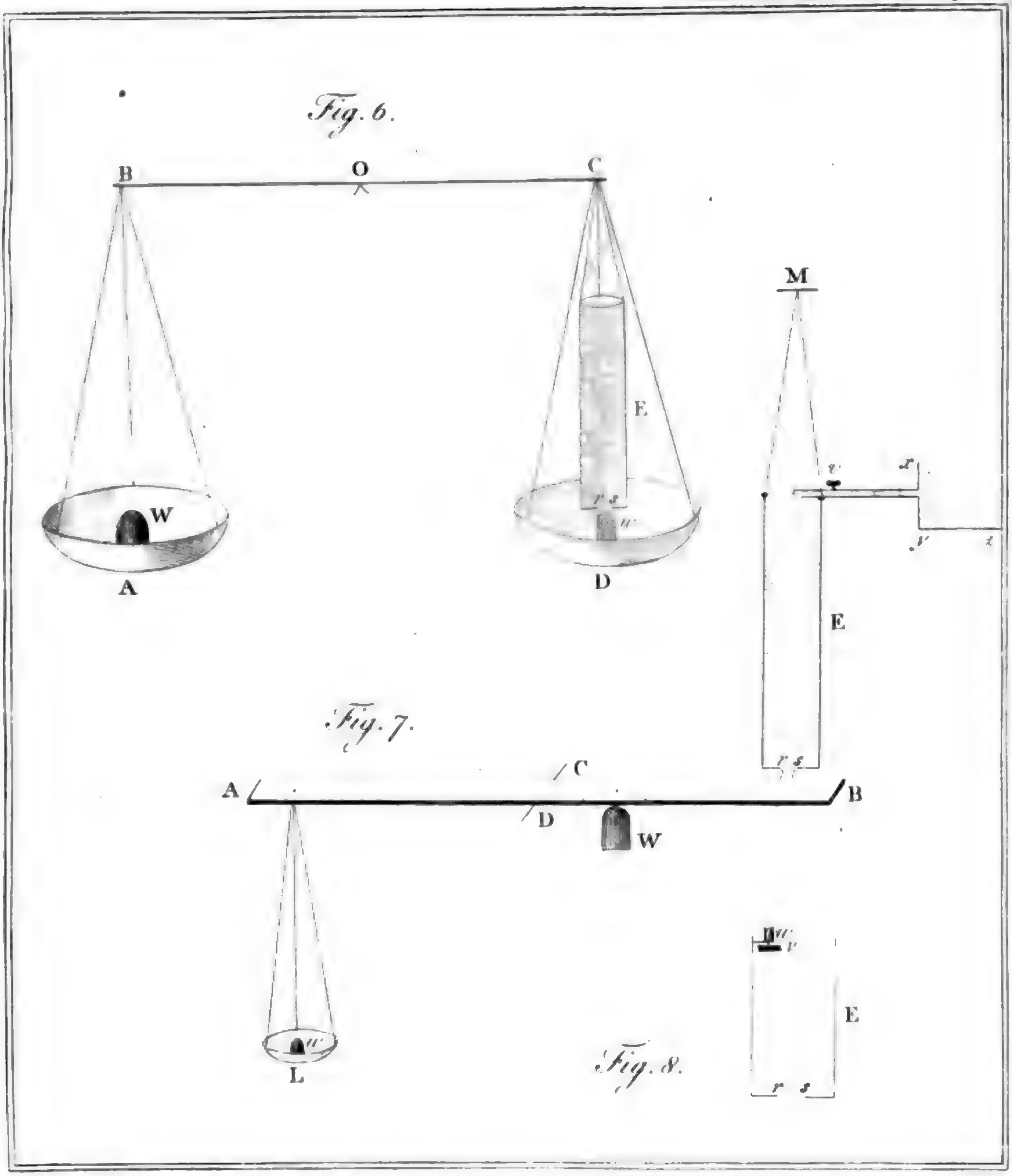


Fig. 6.
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Fig. 7.
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Fig. 8.
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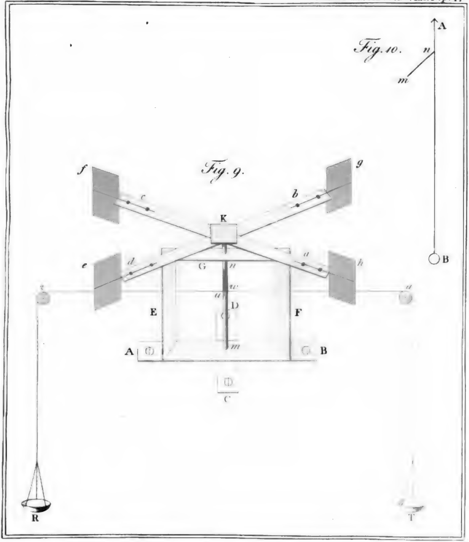
Fig. 10.
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Fig. 9.
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resistances with these two velocities of 1 : 2 will be as W : Q. If R be the sum of the two equal weights put into the scales to give an uniform velocity three times as great as that of the first, then with velocities as 1 : 3 the resistances will be as W : R; and so on. This method was proposed by Mr. ROBINS, in order to determine the law of resistance in terms of the velocity. If the planes be set at any angle, we can by this means get, in terms of the velocity, the law of resistance not only in the direction of the motion of the planes, but also in a direction perpendicular to that of their motion. An account of all the experiments which can be made by this machine, some of which I believe have never yet been attempted, I shall lay before the Royal Society at a future opportunity.

III. *On the Nature and Construction of the Sun and fixed Stars.* *By* William Herschel, *LL.D. F. R. S.*

Read December 18, 1794.

Among the celestial bodies the sun is certainly the first which should attract our notice. It is a fountain of light that illuminates the world! it is the cause of that heat which maintains the productive power of nature, and makes the earth a fit habitation for man! it is the central body of the planetary system; and what renders a knowledge of its nature still more interesting to us is, that the numberless stars which compose the universe, appear, by the strictest analogy, to be similar bodies. Their innate light is so intense, that it reaches the eye of the observer from the remotest regions of space, and forcibly claims his notice.

Now, if we are convinced that an inquiry into the nature and properties of the sun is highly worthy of our notice, we may also with great satisfaction reflect on the considerable progress that has already been made in our knowledge of this eminent body. It would require a long detail to enumerate all the various discoveries which have been made on this subject; I shall, therefore, content myself with giving only the most capital of them.

Sir Isaac Newton has shewn that the sun, by its attractive power, retains the planets of our system in their orbits. He

has also pointed out the method whereby the quantity of matter it contains may be accurately determined. Dr. BRADLEY has assigned the velocity of the solar light with a degree of precision exceeding our utmost expectation. GALILEO, SCHEINER, HEVELIUS, CASSINI, and others, have ascertained the rotation of the sun upon its axis, and determined the position of its equator. By means of the transit of Venus over the disc of the sun, our mathematicians have calculated its distance from the earth; its real diameter and magnitude; the density of the matter of which it is composed; and the fall of heavy bodies on its surface.

From the particulars here enumerated, it is sufficiently obvious, that we have already a very clear idea of the vast importance, and powerful influence of the sun on its planetary system. And if we add to this the beneficent effects we feel on this globe from the diffusion of the solar rays; and consider that, by well traced analogies, the same effects have been proved to take place on other planets of this system; I should not wonder if we were induced to think that nothing remained to be added in order to complete our knowledge: and yet it will not be difficult to shew that we are still very ignorant, at least with regard to the internal construction of the sun. The various conjectures, which have been formed on this subject, are evident marks of the uncertainty under which we have hitherto laboured.

The dark spots in the sun, for instance, have been supposed to be solid bodies revolving very near its surface. They have been conjectured to be the smoke of volcanoes, or the scum floating upon an ocean of fluid matter. They have also been taken for clouds. They were explained to be opaque masses,

swimming in the fluid matter of the sun; dipping down occasionally. It has been supposed that a fiery liquid surrounded the sun, and that, by its ebbing and flowing, the highest parts of it were occasionally uncovered, and appeared under the shape of dark spots; and that, by the return of this fiery liquid, they were again covered, and in that manner successively assumed different phases. The sun itself has been called a globe of fire, though perhaps metaphorically. The waste it would undergo by a gradual consumption, on the supposition of its being ignited, has been ingeniously calculated. And in the same point of view, its immense power of heating the bodies of such comets as draw very near to it has been assigned. The bright spots, or faculæ, have been called clouds of light, and luminous vapours. The light of the sun itself has been supposed to be directly invisible, and not to be perceived unless by reflection; though the proofs, which are brought in support of that opinion, seem to me to amount to no more than, what is sufficiently evident, that we cannot see when rays of light do not enter the eye.

But it is time to profit by the many valuable observations that we are now in possession of. A list of successive eminent astronomers may be named, from GALILEO down to the present time; who have furnished us with materials for examination.

In supporting the ideas I shall propose in this paper, with regard to the physical construction of the sun, I have availed myself of the labours of all these astronomers, but have been induced thereto only by my own actual observation of the solar phænomena; which, besides verifying those particulars that had been already observed, gave me such views of the

solar regions as led to the foundation of a very rational system. For, having the advantage of former observations, my latest reviews of the body of the sun were immediately directed to the most essential points; and the work was by this means facilitated, and contracted into a pretty narrow compass.

The following is a short extract of my observations on the sun, to which I have joined the consequences I now believe myself entitled to draw from them. When all the reasonings on the several phænomena are put together, and a few additional arguments, taken from analogy, which I shall also add, are properly considered, it will be found that a general conclusion may be made which seems to throw a considerable light upon our present subject.

In the year 1779, there was a spot on the sun which was large enough to be seen with the naked eye. By a view of it with a 7-feet reflector, charged with a very high power, it appeared to be divided into two parts. The largest of the two, on the 19th of April, measured 1′ 8″,06 in diameter; which is equal, in length, to more than 31 thousand miles. Both together must certainly have extended above 50 thousand.

The idea of its being occasioned by a volcanic explosion, violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus restore the sun, in that place, to its former splendour, ought to be rejected on many accounts. To mention only one, the great extent of the spot is very unfavourable to that supposition. Indeed a much less violent and less pernicious cause may be assigned, to account for all the appearances of the spot. When we see a dark belt near the equator of the planet Jupiter, we do not recur to earthquakes and volcanoes for its origin. An atmosphere, with

its natural changes, will explain such belts. Our spot in the sun may be accounted for on the same principles. The earth is surrounded by an atmosphere, composed of various elastic fluids. The sun also has its atmosphere, and if some of the fluids which enter into its composition should be of a shining brilliancy, in the manner that will be explained hereafter, while others are merely transparent, any temporary cause which may remove the lucid fluid will permit us to see the body of the sun through the transparent ones. If an observer were placed on the moon, he would see the solid body of our earth only in those places where the transparent fluids of our atmosphere would permit him. In others, the opaque vapours would reflect the light of the sun, without permitting his view to penetrate to the surface of our globe. He would probably also find that our planet had occasionally some shining fluids in its atmosphere; as, not unlikely, some of our northern lights might not escape his notice, if they happened in the unenlightened part of the earth, and were seen by him in his long dark night. Nay, we have pretty good reason to believe, that probably all the planets emit light in some degree; for the illumination which remains on the moon in a total eclipse cannot be entirely ascribed to the light which may reach it by the refraction of the earth's atmosphere. For instance in the eclipse of the moon, which happened October 22, 1790, the rays of the sun refracted by the atmosphere of the earth towards the moon, admitting the mean horizontal refraction to be 30′ 50″,8, would meet in a focus above 189 thousand miles beyond the moon; so that consequently there could be no illumination from rays refracted by our atmosphere. It is, however, not improbable, that about the polar regions of the

earth there may be refraction enough to bring some of the solar rays to a shorter focus. The distance of the moon at the time of the eclipse would require a refraction of 54′ 6″ equal to its horizontal parallax at that time, to bring them to a focus so as to throw light on the moon.

The unenlightened part of the planet Venus has also been seen by different persons, and not having a satellite, those regions that are turned from the sun cannot possibly shine by a borrowed light; so that this faint illumination must denote some phosphoric quality of the atmosphere of Venus.

In the instance of our large spot on the sun, I concluded from appearances that I viewed the real solid body of the sun itself, of which we rarely see more than its shining atmosphere.

In the year 1783, I observed a fine large spot, and followed it up to the edge of the sun's limb. Here I took notice that the spot was plainly depressed below the surface of the sun; and that it had very broad shelving sides. I also suspected some part, at least, of the shelving sides to be elevated above the surface of the sun; and observed that, contrary to what usually happens, the margin of that side of the spot, which was farthest from the limb, was the broadest.

The luminous shelving sides of a spot may be explained by a gentle and gradual removal of the shining fluid, which permits us to see the globe of the sun. As to the uncommon appearance of the broadest margin being on that side of the spot which was farthest from the limb when the spot came near the edge of it, we may surmise that the sun has inequalities on its surface, which may possibly be the cause of it. For, when mountainous countries are exposed, if it should chance that the highest parts of the landscape are situated so as to be near that

side of the margin, or penumbra of the spot, which is towards the limb, it may partly intercept our view of it, when the spot is seen very obliquely. This would require elevations at least five or six hundred miles high; but considering the great attraction exerted by the sun upon bodies at its surface, and the slow revolution it has upon its axis, we may readily admit inequalities to that amount. From the centrifugal force at the sun's equator, and the weight of bodies at its surface, I compute that the power of throwing down a mountain by the exertion of the former, balanced by the superior force of keeping it in its situation of the latter, is near six and a half times less on the sun than on our equatorial regions; and as an elevation similar to one of three miles on the earth would not be less than 334 miles on the sun, there can be no doubt but that a mountain much higher would stand very firmly. The little density of the solar body seems also to be in favour of the height of its mountains; for, *cæteris paribus*, dense bodies will sooner come to their level than rare ones. The difference in the vanishing of the shelving side, instead of explaining it by mountains, may also, and perhaps more satisfactorily, be accounted for from the real difference of the extent, the arrangement, the height, and the intensity of the shining fluid, added to the occasional changes that may happen in these particulars, during the time in which the spot approaches to the edge of the disc. However, by admitting large mountains on the surface of the sun, we shall account for the different opinions of two eminent astronomers; one of whom believed the spots depressed below the sun, while the other supposed them elevated above it. For it is not improbable that some of the solar mountains may be high enough occasionally to project above

the shining elastic fluid, when, by some agitation or other cause, it is not of the usual height; and this opinion is much strengthened by the return of some remarkable spots, which served CASSINI to ascertain the period of the sun's rotation. A very high country, or chain of mountains, may oftener become visible, by the removal of the obstructing fluid, than the lower regions, on account of its not being so deeply covered with it.

In the year 1791, I examined a large spot in the sun, and found it evidently depressed below the level of the surface; about the dark part was a broad margin, or plane of considerable extent, less bright than the sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface.

In confirmation of these appearances, I carefully remarked that the disc of the sun was visibly convex; and the reason of my attention to this particular, was my being already long acquainted with a certain optical deception, that takes place now and then when we view the moon; which is, that all the elevated spots on its surface will seem to be cavities, and all cavities will assume the shape of mountains. But then, at the same time the moon, instead of having the convex appearance of a globe, will seem to be a large concave portion of an hollow sphere. As soon as, by the force of imagination, you drive away the fallacious appearance of a concave moon, you restore the mountains to their protuberance, and sink the cavities again below the level of the surface. Now, when I saw the spot lower than the shining matter of the sun, and an extended plane, also depressed, with shelving sides rising up to the level, I also found that the sun was convex, and appeared in

its natural globular state. Hence I conclude that there could be no deception in those appearances.

How very ill would this observation agree with the ideas of solid bodies bobbing up and down in a fiery liquid? with the smoke of volcanoes, or scum upon an ocean? And how easily it is explained upon our foregoing theory. The removal of the shining atmosphere, which permits us to see the sun, must naturally be attended with a gradual diminution on its borders; an instance of a similar kind we have daily before us, when through the opening of a cloud we see the sky, which generally is attended by a surrounding haziness of some short extent; and seldom transits, from a perfect clearness, at once to the greatest obscurity.

Aug. 26, 1792. I examined the sun with several powers, from 90 to 500. It appears evidently that the black spots are the opaque ground, or body of the sun; and that the luminous part is an atmosphere, which, being interrupted or broken, gives us a transient glimpse of the sun itself. My 7-feet reflector, which is in high perfection, represents the spots, as it always used to do, much depressed below the surface of the luminous part.

Sept. 2, 1792. I saw two spots in the sun with the naked eye. In the telescope I found they were clusters of spots, with many scattered ones besides. Every one of them was certainly below the surface of the luminous disc.

Sept. 8, 1792. Having made a small speculum, merely brought to a perfect figure upon hones, without polish, I found, that by stifling a great part of the solar rays, my object speculum would bear a greater aperture; and thus enabled me to see with more comfort, and less danger. The surface of

the sun was unequal; many parts of it being elevated, and others depressed. This is here to be understood of the shining surface only, as the real body of the sun can probably be seldom seen, otherwise than in its black spots.

It may not be impossible, as light is a transparent fluid, that the sun's real surface also may now and then be perceived; as we see the shape of the wick of a candle through its flame, or the contents of a furnace in the midst of the brightest glare of it; but this, I should suppose, will only happen where the lucid matter of the sun is not very accumulated.

Sept. 9, 1792. I found one of the dark spots in the sun drawn pretty near the preceding edge. In its neighbourhood I saw a great number of elevated bright places, making various figures: I shall call them faculæ, with HEVELIUS; but without assigning to this term any other meaning than what it will hereafter appear ought to be given to it. I see these faculæ extended, on the preceding side, over about one-sixth part of the sun; but so far from resembling torches, they appear to me like the shrivelled elevations upon a dried apple, extended in length, and most of them are joined together, making waves, or waving lines.

By some good views in the afternoon, I find that the rest of the surface of the sun does not contain any faculæ, except a few on the following, and equatorial part of the sun. Towards the north and south I see no faculæ; there is all over the sun a great unevenness in the surface, which has the appearance of a mixture of small points of an unequal light; but they are evidently an unevenness or roughness of high and low parts.

Sept. 11, 1792. The faculæ, in the preceding part of the

sun, are much gone out of the disc, and those in the following are come on. A dark spot also is come on with them.

Sept. 13, 1792. There are a great number of faculæ on the equatorial part of the sun, towards the preceding and following parts. I cannot see any towards the poles; but a roughness is visible every where.

Sept. 16, 1792. The sun contains many large faculæ, on the following side of its equator, and also several on the preceding side. I perceive none about the poles. They seem generally to accompany the spots, and probably, as the faculæ certainly are elevations, a great number of them may occasion neighbouring depressions: that is to say, dark spots.

The faculæ being elevations, very satisfactorily explains the reason why they disappear towards the middle of the sun, and re-appear on the other margin; for, about the place where we lose them, they begin to be edge-ways to our view; and if between the faculæ should lie dark spots, they will most frequently break out in the middle of the sun, because they are no longer covered by the side views of these faculæ.

Sept. 22, 1792. There are not many faculæ in the sun, and but few spots; the whole disc, however, is very much marked with roughness, like an orange. Some of the lowest parts of the inequalities are blackish.

Sept. 23, 1792. The following side of the sun contains many faculæ, near the limb. They take up an arch of about 50 degrees. There are, likewise, some on the preceding side. The north and south is rough as usual; but differently disposed. The faculæ are ridges of elevations above the rough surface.

Feb. 23, 1794. By an experiment I have just now tried, I find it confirmed that the sun cannot be so distinctly viewed

with a small aperture and faint darkening glasses, as with a large aperture and stronger ones; this latter is the method I always use.

One of the black spots on the preceding margin, which was greatly below the surface of the sun, had, next to it, a protuberant lump of shining matter, a little brighter than the rest of the sun.

About all the spots, the shining matter seems to have been disturbed; and is uneven, lumpy, and zig-zagged in an irregular manner.

I call the spots black, not that they are entirely so, but merely to distinguish them; for there is not one of them, to-day, which is not partly, or entirely, covered over with whitish and unequally bright nebulosity, or cloudiness. This, in many of them, comes near to an extinction of the spot; and in others, seems to bring on a subdivision.

Sept. 28, 1794. There is a dark spot in the sun on the following side. It is certainly depressed below the shining atmosphere, and has shelving sides of shining matter, which rise up higher than the general surface, and are brightest at the top. The preceding shelving side is rendered almost invisible, by the overhanging of the preceding elevations; while the following is very well exposed: the spot being apparently such in figure as denotes a circular form, viewed in an oblique direction.

Near the following margin are many bright elevations, close to visible depressions. The depressed parts are less bright than the common surface.

The penumbra, as it is called, about this spot, is a consi-

derable plane, of less brightness than the common surface, and seems to be as much depressed below that surface as the spot is below the plane.

Hence, if the brightness of the sun is occasioned by the lucid atmosphere, the intensity of the brightness must be less where it is depressed; for light, being transparent, must be the more intense the more it is deep.

Oct. 12, 1794. The whole surface of the sun is diversified by inequality in the elevation of the shining atmosphere. The lowest parts are every where darkest; and every little pit has the appearance of a more or less dark spot.

A dark spot, which is on the preceding side, is surrounded by very great inequalities in the elevation of the lucid atmosphere; and its depression below the same is bounded by an immediate rising of very bright light.

Oct. 13, 1794. The spot in the sun I observed yesterday is drawn so near the margin, that the elevated side of the following part of it hides all the black ground, and still leaves the cavity visible, so that the depression of the black spots, and the elevation of the faculæ, are equally evident.

It will now be easy to bring the result of these observations into a very narrow compass. That the sun has a very extensive atmosphere cannot be doubted; and that this atmosphere consists of various elastic fluids, that are more or less lucid and transparent, and of which the lucid one is that which furnishes us with light, seems also to be fully established by all the phænomena of its spots, of the faculæ, and of the lucid surface itself. There is no kind of variety in these appearances

but what may be accounted for with the greatest facility, from the continual agitation which we may easily conceive must take place in the regions of such extensive elastic fluids.

It will be necessary, however, to be a little more particular, as to the manner in which I suppose the lucid fluid of the sun to be generated in its atmosphere. An analogy that may be drawn from the generation of clouds in our own atmosphere, seems to be a very proper one, and full of instruction. Our clouds are probably decompositions of some of the elastic fluids of the atmosphere itself, when such natural causes, as in this grand chemical laboratory are generally at work, act upon them; we may therefore admit that in the very extensive atmosphere of the sun, from causes of the same nature, similar phænomena will take place; but with this difference, that the continual and very extensive decompositions of the elastic fluids of the sun, are of a phosphoric nature, and attended with lucid appearances, by giving out light.

If it should be objected, that such violent and unremitting decompositions would exhaust the sun, we may recur again to our analogy, which will furnish us with the following reflections. The extent of our own atmosphere, we see, is still preserved, notwithstanding the copious decompositions of its fluids, in clouds and falling rain; in flashes of lightning, in meteors, and other luminous phænomena; because there are fresh supplies of elastic vapours, continually ascending to make good the waste occasioned by those decompositions. But it may be urged, that the case with the decomposition of the elastic fluids in the solar atmosphere would be very different, since light is emitted, and does not return to the sun, as clouds do to the earth when they descend in showers of rain. To

which I answer, that in the decomposition of phosphoric fluids every other ingredient but light may also return to the body of the sun. And that the emission of light must waste the sun, is not a difficulty that can be opposed to our hypothesis. For as it is an evident fact that the sun does emit light, the same objection, if it could be one, would equally militate against every other assignable way to account for the phænomenon.

There are moreover considerations that may lessen the pressure of this alleged difficulty. We know the exceeding subtilty of light to be such, that in ages of time its emanation from the sun cannot very sensibly lessen the size of this great body. To this may be added, that, very possibly, there may also be ways of restoration to compensate for what is lost by the emission of light; though the manner in which this can be brought about should not appear to us. Many of the operations of nature are carried on in her great laboratory, which we cannot comprehend; but now and then we see some of the tools with which she is at work. We need not wonder that their construction should be so singular as to induce us to confess our ignorance of the method of employing them, but we may rest assured that they are not a mere *lusus naturæ*. I allude to the great number of small telescopic comets that have been observed; and to the far greater number still that are probably much too small for being noticed by our most diligent searchers after them. Those six, for instance, which my sister has discovered, I can from examination affirm had not the least appearance of any solid nucleus, and seemed to be mere collections of vapours condensed about a centre. Five more, that I have also observed, were nearly of the same nature.

This throws a mystery over their destination, which seems to place them in the allegorical view of tools, probably designed for some salutary purposes to be wrought by them; and, whether the restoration of what is lost to the sun by the emission of light, the possibility of which we have been mentioning above, may not be one of these purposes, I shall not presume to determine. The motion of the comet discovered by Mr. MESSIER in June, 1770, plainly indicated how much its orbit was liable to be changed, by the perturbations of the planets; from which, and the little agreement that can be found between the elements of the orbits of all the comets that have been observed, it appears clearly that they may be directed to carry their salutary influence to any part of the heavens.

My hypothesis, however, as before observed, does not lay me under any obligation to explain how the sun can sustain the waste of light, nor to shew that it will sustain it for ever; and I should also remark that, as in the analogy of generating clouds I merely allude to their production as owing to a decomposition of some of the elastic fluids of our atmosphere, that analogy, which firmly rests upon the fact, will not be less to my purpose to whatever cause these clouds may owe their origin. It is the same with the lucid clouds, if I may so call them, of the sun. They plainly exist, because we see them; the manner of their being generated may remain an hypothesis; and mine, till a better can be proposed, may stand good; but whether it does or not, the consequences I am going to draw from what has been said will not be affected by it.

Before I proceed, I shall only point out, that according to the above theory, a dark spot in the sun is a place in its atmosphere which happens to be free from luminous decomposi-

tions; and that faculæ are, on the contrary, more copious mixtures of such fluids as decompose each other. The penumbra which attends the spots, being generally depressed more or less to about half way between the solid body of the sun and the upper part of those regions in which luminous decompositions take place, must of course be fainter than other parts. No spot favourable for taking measures having lately been on the sun, I can only judge, from former appearances, that the regions in which the luminous solar clouds are formed, adding thereto the elevation of the faculæ, cannot be less than 1843, nor much more than 2765 miles in depth. It is true that in our atmosphere the extent of the clouds is limited to a very narrow compass; but we ought rather to compare the solar ones to the luminous decompositions which take place in our *aurora borealis*, or luminous arches, which extend much farther than the cloudy regions. The density of the luminous solar clouds, though very great, may not be exceedingly more so than that of our *aurora borealis*. For, if we consider what would be the brilliancy of a space two or three thousand miles deep, filled with such corruscations as we see now and then in our atmosphere, their apparent intensity, when viewed at the distance of the sun, might not be much inferior to that of the lucid solar fluid.

From the luminous atmosphere of the sun I proceed to its opaque body, which by calculation from the power it exerts upon the planets we know to be of great solidity; and from the phænomena of the dark spots, many of which, probably on account of their high situations, have been repeatedly seen, and otherwise denote inequalities in their level, we surmise that its surface is diversified with mountains and vallies.

What has been said enables us to come to some very important conclusions, by remarking, that this way of considering the sun and its atmosphere, removes the great dissimilarity we have hitherto been used to find between its condition and that of the rest of the great bodies of the solar system.

The sun, viewed in this light, appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or in strictness of speaking, the only primary one of our system; all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface; the rotation upon its axis, and the fall of heavy bodies, leads us on to suppose that it is most probably also inhabited, like the rest of the planets, by beings whose organs are adapted to the peculiar circumstances of that vast globe.

Whatever fanciful poets might say, in making the sun the abode of blessed spirits, or angry moralists devise, in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had any other foundation for their assertions than mere opinion and vague surmise; but now I think myself authorized, *upon astronomical principles,* to propose the sun as an inhabitable world, and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it.

It may, however, not be amiss to remove a certain difficulty, which arises from the effect of the sun's rays upon our globe. The heat which is here, at the distance of 95 millions of miles, produced by these rays, is so considerable, that it may be objected, that the surface of the globe of the sun itself must be scorched up beyond all conception.

This may be very substantially answered by many proofs drawn from natural philosophy, which shew that heat is produced by the sun's rays only when they act upon a calorific medium; they are the cause of the production of heat, by uniting with the matter of fire, which is contained in the substances that are heated: as the collision of flint and steel will inflame a magazine of gunpowder, by putting all the latent fire it contains into action. But an instance or two of the manner in which the solar rays produce their effect, will bring this home to our most common experience.

On the tops of mountains of a sufficient height, at an altitude where clouds can very seldom reach, to shelter them from the direct rays of the sun, we always find regions of ice and snow. Now if the solar rays themselves conveyed all the heat we find on this globe, it ought to be hottest where their course is least interrupted. Again, our aëronauts all confirm the coldness of the upper regions of the atmosphere; and since, therefore, even on our earth the heat of any situation depends upon the aptness of the medium to yield to the impression of the solar rays, we have only to admit, that on the sun itself, the elastic fluids composing its atmosphere, and the matter on its surface, are of such a nature as not to be capable of any excessive affection from its own rays; and, indeed, this seems to be proved by the copious emission of them; for if the elastic fluids of the atmosphere, or the matter contained on the surface of the sun, were of such a nature as to admit of an easy, chemical combination with its rays, their emission would be much impeded.

Another well known fact is, that the solar focus of the largest lens, thrown into the air, will occasion no sensible heat in the place where it has been kept for a considerable

time, although its power of exciting combustion, when proper bodies are exposed, should be sufficient to fuse the most refractory substances.*

It will not be necessary to mention other objections, as I can think of none that may be made, but what a proper consideration of the foregoing observations will easily remove; such as may be urged from the dissimilarity between the luminous atmosphere of the sun and that of our globe will be touched upon hereafter, when I consider the objections that may be assigned against the moon's being an inhabitable satellite.

I shall now endeavour, by analogical reasonings, to support the ideas I have suggested concerning the construction and purposes of the sun; in order to which, it will be necessary to begin with such arguments as the nature of the case will admit, to shew that our moon is probably inhabited. This satellite is of all the heavenly bodies the nearest, and therefore most within the reach of our telescopes. Accordingly we find, by repeated inspection, that we can with perfect confidence give the following account of it.

It is a secondary planet, of a considerable size; the surface of which is diversified, like that of the earth, by mountains and vallies. Its situation, with respect to the sun, is much like that of the earth; and, by a rotation on its axis, it enjoys an agreeable variety of seasons, and of day and night. To the moon, our globe will appear to be a very capital satellite;

* The subject of light and heat has been very ably discussed by Mr. DE LUC, in his excellent work, *Idées sur la Météorologie*, Tome I. part 2, chap. 2, section 2, *De la Nature du Feu*; and Tome II. part 3, chap. 6, section 1, *Des Rapports de la Lumière avec la Chaleur dans l'Atmosphère.*

undergoing the same regular changes of illuminations as the moon does to the earth. The sun, the planets, and the starry constellations of the heavens, will rise and set there as they do here; and heavy bodies will fall on the moon as they do on the earth. There seems only to be wanting, in order to complete the analogy, that it should be inhabited like the earth.

To this it may be objected, that we perceive no large seas in the moon; that its atmosphere (the existence of which has even been doubted by many) is extremely rare, and unfit for the purposes of animal life; that its climates, its seasons, and the length of its days, totally differ from ours; that without dense clouds (which the moon has not), there can be no rain; perhaps no rivers, no lakes. In short, that, notwithstanding the similarity which has been pointed out, there seems to be a decided difference in the two planets we have compared.

My answer to this will be, that that very difference which is now objected, will rather strengthen the force of my argument than lessen its value: we find, even upon our globe, that there is the most striking difference in the situation of the creatures that live upon it. While man walks upon the ground, the birds fly in the air, and fishes swim in water; we can certainly not object to the conveniences afforded by the moon, if those that are to inhabit its regions are fitted to their conditions as well as we on this globe are to ours. An absolute, or total sameness, seems rather to denote imperfections, such as nature never exposes to our view; and, on this account, I believe the analogies that have been mentioned fully sufficient to establish the high probability of the moon's being inhabited like the earth.

To proceed, we will now suppose an inhabitant of the moon,

who has not properly considered such analogical reasonings as might induce him to surmise that our earth is inhabited, were to give it as his opinion that the use of that great body, which he sees in his neighbourhood, is to carry about his little globe, that it may be properly exposed to the light of the sun, so as to enjoy an agreeable and useful variety of illumination, as well as to give it light by reflection from the sun, when direct daylight cannot be had. Suppose also that the inhabitants of the satellites of Jupiter, Saturn, and the Georgian planet, were to look upon the primary ones, to which they belong, as mere attractive centres, to keep together their orbits, to direct their revolution round the sun, and to supply them with reflected light in the absence of direct illumination. Ought we not to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the earth, and those other planets that have satellites about them, perform all the offices that have been named, for the inhabitants of these little globes; but to us, who live upon one of these planets, their reasonings cannot but appear very defective; when we see what a magnificent dwelling place the earth affords to numberless intelligent beings.

These considerations ought to make the inhabitants of the planets wiser than we have supposed those of their satellites to be. We surely ought not, like them, to say "the sun (that "immense globe, whose body would much more than fill the "whole orbit of the moon) is merely an attractive centre to "us." From experience we can affirm, that the performance of the most salutary offices to inferior planets, is not inconsistent with the dignity of superior purposes; and, in consequence of such analogical reasonings, assisted by telescopic

views, which plainly favour the same opinion, we need not hesitate to admit that the sun is richly stored with inhabitants.

This way of considering the sun is of the utmost importance in its consequences. That stars are suns can hardly admit of a doubt. Their immense distance would perfectly exclude them from our view, if the light they send us were not of the solar kind. Besides, the analogy may be traced much farther. The sun turns on its axis. So does the star Algol. So do the stars called β Lyræ, δ Cephei, η Antinoi, ο Ceti, and many more; most probably all. From what other cause can we so probably account for their periodical changes? Again, our sun has spots on its surface. So has the star Algol; and so have the stars already named; and probably every star in the heavens. On our sun these spots are changeable. So they are on the star ο Ceti; as evidently appears from the irregularity of its changeable lustre, which is often broken in upon by accidental changes, while the general period continues unaltered. The same little deviations have been observed in other periodical stars, and ought to be ascribed to the same cause. But if stars are suns, and suns are inhabitable, we see at once what an extensive field for animation opens itself to our view.

It is true that analogy may induce us to conclude, that since stars appear to be suns, and suns, according to the common opinion, are bodies that serve to enlighten, warm, and sustain a system of planets, we may have an idea of numberless globes that serve for the habitation of living creatures. But if these suns themselves are primary planets, we may see some thousands of them with our own eyes; and millions by the help of telescopes; when at the same time, the same analogical

reasoning still remains in full force, with regard to the planets which these suns may support.

In this place I may, however, take notice that, from other considerations, the idea of suns or stars being *merely* the supporters of systems of planets, is not absolutely to be admitted as a general one. Among the great number of very compressed clusters of stars, I have given in my catalogues, there are some which open a different view of the heavens to us. The stars in them are so very close together, that, notwithstanding the great distance at which we may suppose the cluster itself to be, it will hardly be possible to assign any sufficient mutual distance to the stars composing the cluster, to leave room for crowding in those planets, for whose support these stars have been, or might be, supposed to exist. It should seem, therefore, highly probable that they exist for themselves; and are, in fact, only very capital, *lucid*, primary planets, connected together in one great system of mutual support.

As in this argument I do not proceed upon conjectures, but have actual observations in view, I shall mention an instance in the clusters, No. 26, 28, and 35, VI. class, of my catalogue of nebulæ, and clusters of stars. (See Phil. Trans. Vol. LXXIX. Part II. p. 251.) The stars in them are so crowded, that I cannot conjecture them to be at a greater apparent distance from each other than five seconds; even after a proper allowance for such stars, as on a supposition of a globular form of the cluster, will interfere with one another, has been made. Now, if we would leave as much room between each of these stars as there is between the sun and Sirius, we must place these clusters 42104 times as far from us as that star is from the sun. But in order to bring down the lustre of Sirius to

that of an equal star placed at such a distance, I ought to reduce the aperture of my 20-feet telescope to less than the two-and-twenty hundredth part of an inch; when certainly I could no longer expect to see any star at all.

The same remark may be made, with regard to the number of very close double stars; whose apparent diameters being alike, and not very small, do not indicate any very great mutual distance. From which, however, must be deducted all those where the different distances may be compensated by the real difference in their respective magnitudes.

To what has been said may be added, that in some parts of the milky way, where yet the stars are not very small, they are so crowded, that in the year 1792, Aug. 22, I found by the gages that, in 41 minutes of time, no less than 258 thousand of them had passed through the field of view of my telescope.*

It seems, therefore, upon the whole not improbable that, in

* The star-gages ran thus:

From	$19^h\ 35'$	to	$19^h\ 51'$	600	stars in the field
	19 51	—	19 57	440	
	19 57	—	20 12	360	
	20 12	—	20 16	260	

The breadth of the sweep was $2^\circ\ 35'$, the diameter of the field $15'$, and the mean polar distance $73^\circ\ 54'$. Then let

F, be the diameter of the field of view,
S, the number of stars in each field,
B, the breadth of the sweep, plus F,
T, the length of the sweep expressed in minutes of space,
φ, the sine of the mean polar distance,
C, the constant fraction ,7854,

and the stars in these four successive short sweeps will be found by the expression $\frac{BTS\varphi}{F^2C}$ equal to 133095. 36601. 74866. 14419. or in all 258981.

many cases, stars are united in such close systems as not to leave much room for the orbits of planets, or comets; and that consequently, upon this account also, many stars, unless we would make them mere useless brilliant points, may themselves be lucid planets, perhaps unattended by satellites.

POSTSCRIPT.

The following observations, which were made with an improved apparatus, and under the most favourable circumstances, should be added to those which have been given. They are decisive with regard to one of the conditions of the lucid matter of the sun.

Nov. 26, 1794. Eight spots in the sun, and several subdivisions of them, are all equally depressed.

The sun is mottled every where.

The mottled appearance of the sun is owing to an inequality in the level of the surface.

The sun is equally mottled at its poles and at its equator; but the mottled appearances may be seen better about the middle of the disc than towards the circumference, on account of the sun's spherical form.

The unevenness arising from the elevation and depression of the mottled appearance on the surface of the sun, seems, in many places, to amount to as much, or to nearly as much as the depression of the penumbræ of the spots below the upper part of the shining substance; without including faculæ, which are protuberant.

The lucid substance of the sun is neither a liquid, nor an

elastic fluid; as is evident from its not instantly filling up the cavities of the spots, and of the unevenness of the mottled parts. It exists, therefore, in the manner of lucid clouds swimming in the transparent atmosphere of the sun; or rather, of luminous decompositions taking place within that atmosphere.

IV. *An Account of the late Eruption of Mount* Vesuvius. *In a Letter from the Right Honourable Sir* William Hamilton, *K. B. F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read January 15, 1795.

SIR, Naples, August 25th, 1794.

EVERY day produces some new publication relative to the late tremendous eruption of mount Vesuvius, so that the various phænomena that attended it will be found on record in either one or other of these publications, and are not in that danger of being passed over and forgotten, as they were formerly, when the study of natural history was either totally neglected, or treated of in a manner very unworthy of the great Author of nature. I am sorry to say, that even so late as in the accounts of the earthquakes in Calabria in 1783, printed at Naples, nature is taxed with being malevolent, and bent upon destruction. In a printed account of another great eruption of Mount Vesuvius in 1631, by ANTONIO SANTORELLI, doctor of medicine, and professor of natural philosophy in the university of Naples, and at the head of the fourth chapter of his book, are these words: *Se questo incendio sia opera de' demonii? Whether this eruption be the work of devils?* The account of an eruption of Vesuvius in 1737, published at Naples by Doctor SERAO, is of a very different cast, and does great honour to his memory. All great eruptions of volcanoes must

naturally produce nearly the same phænomena, and in SERAO's book almost all the phænomena we have been witness to during the late eruption of Vesuvius, are there admirably described, and well accounted for. The classical accounts of the eruption of Vesuvius, which destroyed the towns of Herculaneum and Pompeii, and many of the existing printed accounts of its great eruption in 1631 (although the latter are mixed with puerilities) might pass for an account of the late eruption by only changing the date, and omitting that circumstance of the retreat of the sea from the coast, which happened in both those great eruptions, and not in this; and I might content myself by referring to those accounts, and assuring you at the same time, that the late eruption, after those two, appears to have been the most violent recorded by history, and infinitely more alarming than either the eruption of 1767, or that of 1779, of both of which I had the honour of giving a particular account to the Royal Society. However, I think it my duty rather to hazard being guilty of repetition than to neglect the giving you every satisfaction in my power, relative to the late formidable operation of nature.

You know, Sir, that with the kind assistance of the Father ANTONIO PIAGGI, of the order of the *Scole Pie*, who has resided many years at Resina, on the very foot of Mount Vesuvius, and in the full view of it, I am in possession of an exact diary of that volcano, from the year 1779 to this day, and which is also accompanied with drawings. It is plain, from some remarks in that diary, previous to this eruption, that a great one was expected, and that we were apprehensive of the mischief that might probably attend the falling in of the crater, which had been much contracted within these two years

past, by the great emission of scoriæ and ashes from time to time, and which had also increased the height of the volcano, and nearly filled up its crater. The frequent slight eruptions of lava for some years past have issued from near the summit, and ran in small channels in different directions down the flanks of the mountain, and from running in covered channels, had often an appearance as if they came immediately out of the sides of Vesuvius, but such lavas had not sufficient force to reach the cultivated parts atthe foot of the mountain. In the year 1779, the whole quantity of the lava in fusion having been at once thrown up with violence out of the crater of Vesuvius, and a great part of it falling, and cooling on its cone, added much to the solidity of the walls of this huge natural chimney, if I may be allowed so to call it, and has not of late years allowed of a sufficient discharge of lava to calm that fermentation, which by the subterraneous noises heard at times, and by the explosions of scoriæ and ashes, was known to exist within the bowels of the volcano; so that the eruptions of late years, before this last, have, as I have said, been simply from the lava having boiled over the crater, the sides being sufficiently strong to confine it, and oblige it to rise and overflow. The mountain had been remarkably quiet for seven months before its late eruption, nor did the usual smoke issue from its crater, but at times it emitted small clouds of smoke, that floated in the air in the shape of little trees. It was remarked by the Father Antonio di Petrizzi, a capuchin friar (who has printed an account of the late eruption) from his convent close to the unfortunate town of Torre del Greco, that for some days preceding this eruption a thick vapour was seen to surround the mountain, about a quarter of a mile

beneath its crater, as it was remarked by him, and others at the same time, that both the sun and the moon had often an unusual reddish cast.

The water of the great fountain at Torre del Greco began to decrease some days before the eruption, so that the wheels of a corn-mill, worked by that water, moved very slowly; it was necessary in all the other wells of the town and its neighbourhood to lengthen the ropes daily, in order to reach at the water; and some of the wells became quite dry. Although most of the inhabitants were sensible of this phænomenon, not one of them seems to have suspected the true cause of it. It has been well attested, that eight days before the eruption, a man and two boys, being in a vineyard above Torre del Greco (and precisely on the spot where one of the new mouths opened, from whence the principal current of lava that destroyed the town issued), were much alarmed by a sudden puff of smoke that came out of the earth close to them, and was attended with a slight explosion.

Had this circumstance, with that of the subterraneous noises heard at Resina for two days before the eruption (with the additional one of the decrease of water in the wells, as abovementioned) been communicated at the time, it would have required no great foresight to have been certain that an eruption of the volcano was near at hand, and that its force was directed particularly towards that part of the mountain.

On the 12th of June, in the morning, there was a violent fall of rain, and soon after the inhabitants of Resina, situated directly over the ancient town of Herculaneum, were sensible of a rumbling subterraneous noise, which was not heard at Naples.

From the month of January to the month of May last, the atmosphere was generally calm, and we had continued dry weather. In the month of May we had a little rain, but the weather was unusually sultry. For some days preceding the eruption, the Duke DELLA TORRE, a learned and ingenious nobleman of this country, and who has published two letters upon the subject of the late eruption, observed by his electrometers that the atmosphere was charged in excess with the electric fluid, and continued so for several days during the eruption: there are many other curious observations in the duke's account of the late eruption.

About 11 o'clock at night of the 12th of June, at Naples we were all sensible of a violent shock of an earthquake; the undulatory motion was evidently from east to west, and appeared to me to have lasted near half a minute. The sky, which had been quite clear, was soon after covered with black clouds. The inhabitants of the towns and villages, which are very numerous at the foot of Vesuvius, felt this earthquake still more sensibly, and say, that the shock at first was from the bottom upwards, after which followed the undulation from east to west. This earthquake extended all over the Campagna Felice; and their Sicilian Majesties were pleased to tell me, that the royal palace at Caserta, which is 15 miles from this city, and one of the most magnificent and solid buildings in Europe (the walls being 18 feet thick), was shook in such a manner as to cause great alarm, and that all the chamber bells rang. It was likewise much felt at Beneventum, about 30 miles from Naples; and at Ariano in Puglia, which is at a much greater distance; both these towns have been often afflicted with earthquakes.

On Sunday the 15th of June, soon after 10 o'clock at night, another shock of an earthquake was felt at Naples, but did not appear to be quite so violent as that of the 12th, nor did it last so long; at the same moment a fountain of bright fire, attended with a very black smoke and a loud report, was seen to issue, and rise to a great height, from about the middle of the cone of Vesuvius; soon after another of the same kind broke out at some little distance lower down; then, as I suppose by the blowing up of a covered channel full of red-hot lava, it had the appearance as if the lava had taken its course directly up the steep cone of the volcano. Fresh fountains succeeded one another hastily, and all in a direct line tending, for about a mile and a half down, towards the towns of Resina and Torre del Greco. I could count 15 of them, but I believe there were others obscured by the smoke. It seems probable, that all these fountains of fire, from their being in such an exact line, proceeded from one and the same long fissure down the flanks of the mountain, and that the lava and other volcanic matter forced its way out of the widest parts of the crack, and formed there the little mountains and craters that will be described in their proper place. It is impossible that any description can give an idea of this fiery scene, or of the horrid noises that attended this great operation of nature. It was a mixture of the loudest thunder, with incessant reports, like those from a numerous heavy artillery, accompanied by a continued hollow murmur, like that of the roaring of the ocean during a violent storm; and added to these was another blowing noise, like that of the going up of a large flight of sky-rockets, and which brought to my mind also that noise which is produced by the action of the enormous bellows on the furnace of the Carron

iron foundery in Scotland, and which it perfectly resembled. The frequent falling of the huge stones and scoriæ, which were thrown up to an incredible height from some of the new mouths, and one of which having been since measured by the Abbé TATA (who has published an account of this eruption), was 10 feet high, and 35 in circumference, contributed undoubtedly to the concussion of the earth and air, which kept all the houses at Naples for several hours in a constant tremor, every door and window shaking and rattling incessantly, and the bells ringing. This was an awful moment! The sky, from a bright full moon and star-light, began to be obscured; the moon had presently the appearance of being in an eclipse, and soon after was totally lost in obscurity. The murmur of the prayers and lamentations of a numerous populace forming various processions, and parading in the streets, added likewise to the horror. As the lava did not appear to me to have yet a sufficient vent, and it was now evident that the earthquakes we had already felt had been occasioned by the air and fiery matter confined within the bowels of the mountain, and probably at no small depth (considering the extent of those earthquakes), I recommended to the company that was with me, who began to be much alarmed, rather to go and view the mountain at some greater distance, and in the open air, than to remain in the house, which was on the sea side, and in the part of Naples that is nearest and most exposed to Vesuvius. We accordingly went to Posilipo, and viewed the conflagration, now become still more considerable, from the sea side under that mountain; but whether from the eruption having increased, or from the loud reports of the volcanic explosions being repeated by the mountain

behind us, the noise was much louder, and more alarming than that we had heard in our first position, at least a mile nearer to Vesuvius. After some time, and which was about two o'clock in the morning of the 16th, having observed that the lavas ran in abundance freely, and with great velocity, having made a considerable progress towards Resina, the town which it first threatened, and that the fiery vapours which had been confined had now free vent, through many parts of a crack of more than a mile and a half in length, as was evident from the quantity of inflamed matter and black smoke, which continued to issue from the new mouths abovementioned without any interruption, I concluded that at Naples all danger from earthquakes, which had been my greatest apprehension, was now totally removed, and we returned to our former station at S. Lucia at Naples.

All this time there was not the smallest appearance of fire or smoke from the crater on the summit of Vesuvius; but the black smoke and ashes issuing continually from so many new mouths, or craters, formed an enormous and dense body of clouds over the whole mountain, and which began to give signs of being replete with the electric fluid, by exhibiting flashes of that sort of zig-zag lightning, which in the volcanic language of this country is called *ferilli*, and which is the constant attendant on the most violent eruptions. From what I have read and seen, it appears to me, that the truest judgment that can be formed of the degree of force of the fermentation within the bowels of a volcano during its eruption, would be from observing the size, and the greater or less elevation of those piles of smoky clouds, which rise out of the craters, and form a gigantic mass over it, usually in the

form of a pine tree, and from the greater or less quantity of the *ferilli*, or volcanic electricity, with which those clouds appear to be charged.

During thirty years that I have resided at Naples, and in which space of time I have been witness to many eruptions of Vesuvius, of one sort or other, I never saw the gigantic cloud abovementioned replete with the electric fire, except in the two great eruptions of 1767, that of 1779, and during this more formidable one. The electric fire, in the year 1779, that played constantly within the enormous black cloud over the crater of Vesuvius, and seldom quitted it, was exactly similar to that which is produced, on a very small scale, by the conductor of an electrical machine communicating with an insulated plate of glass, thinly spread over with metallic filings, &c. when the electric matter continues to play over it in zigzag lines without quitting it. I was not sensible of any noise attending that operation in 1779; whereas the discharge of the electrical matter from the volcanic clouds during this eruption, and particularly the second and third days, caused explosions like those of the loudest thunder; and indeed the storms raised evidently by the sole power of the volcano, resembled in every respect all other thunder-storms; the lightning falling and destroying every thing in its course. The house of the Marquis of Berio at S. Iorio, situated at the foot of Vesuvius, during one of these volcanic storms was struck with lightning, which having shattered many doors and windows, and damaged the furniture, left for some time a strong smell of sulphur in the rooms it passed through. Out of these gigantic and volcanic clouds, besides the lightning, both during this eruption and that of 1779, I have, with many others,

seen balls of fire issue, and some of a considerable magnitude, which bursting in the air, produced nearly the same effect as that from the air-balloons in fireworks, the electric fire that came out having the appearance of the serpents with which those firework balloons are often filled. The day on which Naples was in the greatest danger from the volcanic clouds, two small balls of fire, joined together by a small link like a chain-shot, fell close to my *casino*, at Posilipo; they separated, and one fell in the vineyard above the house, and the other in the sea, so close to it that I heard a splash in the water; but, as I was writing, I lost the sight of this phænomenon, which was seen by some of the company with me, and related to me as above. The Abbé TATA, in his printed account of this eruption, mentions an enormous ball of this kind which flew out of the crater of Vesuvius whilst he was standing on the edge of it, and which burst in the air at some distance from the mountain, soon after which he heard a noise like the fall of a number of stones, or of a heavy shower of hail.

During the eruption of the 15th at night, few of the inhabitants of Naples, from the dread of earthquakes, ventured to go to their beds. The common people were either employed in devout processions in the streets, or were sleeping on the quays and open places; the nobility and gentry, having caused their horses to be taken from their carriages, slept in them in the squares and open places, or on the high roads just out of the town. For several days, whilst the volcanic storms of thunder and lightning lasted, the inhabitants at the foot of the volcano, both on the sea side and the Somma side, were often sensible of a tremor in the earth, as well as of the concussions

in the air, but at Naples only the earthquakes of the 12th and 15th of June were distinctly and universally felt: this fair city could not certainly have resisted long, had not those earthquakes been fortunately of a short duration. Throughout this eruption, which continued in force about ten days, the fever of the mountain, as has been remarked in former eruptions, shewed itself to be in some measure periodical, and generally was most violent at the break of day, at noon, and at midnight.

About four o'clock in the morning of the 16th, the crater of Vesuvius began to shew signs of being open, by some black smoke issuing out of it; and at daybreak another smoke, tinged with red, issuing from an opening near the crater, but on the other side of the mountain, and facing the town of Ottaiano, shewed that a new mouth had opened there, and from which, as we heard afterwards, a considerable stream of lava issued, and ran with great velocity through a wood, which it burnt; and having run about three miles in a few hours, it stopped before it had arrived at the vineyards and cultivated lands. The crater, and all the conical part of Vesuvius, was soon involved in clouds and darkness, and so it remained for several days; but above these clouds, although of a great height, we could often discern fresh columns of smoke from the crater, rising furiously still higher, until the whole mass remained in the usual form of a pine tree; and in that gigantic mass of heavy clouds the *ferilli*, or volcanic lightning, was frequently visible, even in the day time. About five o'clock in the morning of the 16th we could plainly perceive, that the lava which had first broke out from the several new mouths on the south

side of the mountain, had reached the sea, and was running into it, having overwhelmed, burnt, and destroyed the greatest part of Torre del Greco, the principal stream of lava having taken its course through the very centre of the town. We observed from Naples, that when the lava was in the vineyards in its way to the town, there issued often, and in different parts of it, a bright pale flame, and very different from the deep red of the lava; this was occasioned by the burning of the trees that supported the vines. Soon after the beginning of this eruption, ashes fell thick at the foot of the mountain, all the way from Portici to the Torre del Greco; and what is remarkable, although there were not at that time any clouds in the air, except those of smoke from the mountain, the ashes were wet, and accompanied with large drops of water, which, as I have been well assured, were to the taste very salt; the road, which is paved, was as wet as if there had been a heavy shower of rain. Those ashes were black and coarse, like the sand of the sea shore, whereas those that fell there, and at Naples some days after, were of a light-grey colour, and as fine as Spanish snuff, or powdered bark. They contained many saline particles; as I observed, when I went to the town of Torre del Greco on the 17th of June, that those ashes that lay on the ground, exposed to the burning sun, had a coat of the whitest powder on their surface, which to the taste was extremely salt and pungent. In the printed account of the late eruption by Emanuel Scotti, doctor of physic and professor of philosophy in the university of Naples, he supposes (which appears to be highly probable) that the water which accompanied the fall of the ashes at the beginning of the erup-

tion, was produced by the mixture of the inflammable and dephlogisticated air, according to experiments made by Doctor Priestley and Monsieur Lavoisier.

By the time that the lava had reached the sea, between five and six o'clock in the morning of the 16th, Vesuvius was so completely involved in darkness, that we could no more discern the violent operation of nature that was going on there, and so it remained for several days; but the dreadful noise we heard at times, and the red tinge on the clouds over the top of the mountain, were evident signs of the activity of the fire underneath. The lava ran but slowly at Torre del Greco after it had reached the sea; and on the 17th of June in the morning, when I went in my boat to visit that unfortunate town, its course was stopped, excepting that at times a little rivulet of liquid fire issued from under the smoking scoriæ into the sea, and caused a hissing noise, and a white vapour smoke; at other times, a quantity of large scoriæ were pushed off the surface of the body of the lava into the sea, discovering that it was red hot under that surface; and even to this day the centre of the thickest part of the lava that covers the town retains its red heat. The breadth of the lava that ran into the sea, and has formed a new promontory there, after having destroyed the greatest part of the town of Torre del Greco, having been exactly measured by the Duke della Torre, is of English feet 1204. Its height above the sea is 12 feet, and as many feet under water; so that its whole height is 24 feet; it extends into the sea 626 feet. I observed that the sea water was boiling as in a cauldron, where it washed the foot of this new formed promontory; and although I was at least an hundred yards from it, observing that the sea smoked near my

boat, I put my hand into the water, which was literally scalded; and by this time my boatmen observed that the pitch from the bottom of the boat was melting fast, and floating on the surface of the sea, and that the boat began to leak; we therefore retired hastily from this spot, and landed at some distance from the hot lava. The town of Torre del Greco contained about 18000 inhabitants, all of which (except about 15, who from either age or infirmity could not be moved, and were overwhelmed by the lava in their houses) escaped either to Castel-a-mare, which was the ancient Stabiæ, or to Naples; but the rapid progress of the lava was such, after it had altered its course from Resina, which town it first threatened, and had joined a fresh lava that issued from one of the new mouths in a vineyard, about a mile from the town, that it ran like a torrent over the town of Torre del Greco, allowing the unfortunate inhabitants scarcely time to save their lives; their goods and effects were totally abandoned, and indeed several of the inhabitants, whose houses had been surrounded with lava whilst they remained in them, escaped from them and saved their lives the following day, by coming out of the tops of their houses, and walking over the scoriæ on the surface of the red-hot lava. Five or six old nuns were taken out of a convent in this manner, on the 16th of June, and carried over the hot lava, as I was informed by the friar who assisted them; and who told me that their stupidity was such, as not to have been the least alarmed, or sensible of their danger: he found one of upwards of 90 years of age actually warming herself at a point of red-hot lava, which touched the window of her cell, and which she said was very comfortable; and though now apprized of their danger, they were still very unwilling to leave

the convent, in which they had been shut up almost from their infancy, their ideas being as limited as the space they inhabited. Having desired them to pack up whatever they had that was most valuable, they all loaded themselves with biscuits and sweetmeats, and it was but by accident that the friar discovered that they had left a sum of money behind them, which he recovered for them; and these nuns are now in a convent at Naples.

At the time I landed at Torre del Greco on the 17th, I found some few of its inhabitants returned, and endeavouring to recover their effects from such houses as had not been thrown down, or were not totally buried under the lava; but alas! what was their cruel disappointment when they found that their houses had been already broke open, and completely gutted of every thing that was valuable; and I saw a scuffle at the door of one house, between the proprietors, and the robbers who had taken possession of it. The lava had passed over the centre and best part of the town; no part of the cathedral remained above it, except the upper part of a square brick tower, in which are the bells; and it is a curious circumstance that those bells, although they are neither cracked or melted, are deprived of their tone as much as if they had been cracked, I suppose by the action of the acid and vitriolic vapours of the lava. Some of the inhabitants of Torre del Greco told me, that when the lava first entered the sea, it threw up the water to a prodigious height; and particularly when two points of lava met and inclosed a pool of water, that then that water was thrown up with great violence, and a loud report: they likewise told me, that at this time, as well as the day after, a great many boiled fish were seen floating

on the surface of the sea; and I have since been assured by many of the fishermen of Portici, Torre del Greco, and Torre dell' Annunziata (all of which towns are situated at the foot of Vesuvius), that they could not for many days during the eruption catch a fish within two miles of that coast, which they had evidently deserted.

When this lava is cooled sufficiently, which may not be until some months hence, I shall be curious to examine whether the centre, or solid and compact parts, of the lava that ran into the sea has taken, as it probably may, the prismatical form of basalt columns, like many other ancient lavas disgorged into the water. The exterior of this lava at present, like all others, offers to the eye nothing but a confused heap of loose scoriæ. The lava over the cathedral, and in other parts of the town, is upwards of 40 feet in thickness; the general height of the lava during its whole course is about 12 feet, and in some parts not less than a mile in breadth. I walked in the few remaining streets of the town, and I went on the top of one of the highest houses that was still standing, although surrounded by the lava; I saw from thence distinctly the whole course of the lava, that covered the best part of the town; the tops of the houses were just visible here and there in some parts, and the timbers within still burning caused a bright flame to issue out of the surface; in other parts, the sulphur and salts exhaled in a white smoke from the lava, forming a white or yellow crust on the scoriæ round the spots where it issued with the most force. Often I heard little explosions, and saw that they blew up, like little mines, fragments of the scoriæ and ashes into the air; I suppose them to have been occasioned either by rarefied air in confined cellars, or perhaps

by small portions of gunpowder taking fire, as few in this country are without a gun and some little portion of gunpowder in their houses. As the church feasts are here usually attended with fireworks and crackers, a firework-maker of this town had a very great quantity of fireworks ready made for an approaching feast, and some gunpowder, all of which had been shut up in his house by the lava, a part of which had even entered one of the rooms; yet he actually saved all his fireworks and gunpowder some days after, by carrying them safely over the hot lava. I should not have been so much at my ease had I known of this gunpowder, and of several other barrels that were at the same time in the cellar of another house, inclosed by the lava, and which were afterwards brought off on women's heads, little thinking of their danger, over the scoriæ of the lava, that was red-hot underneath. The heat in the streets of the town, at this time, was so great as to raise the quicksilver of my thermometer to very near 100 degrees, and close to the hot lava it rose much higher; but what drove me from this melancholy spot was, that one of the robbers with a great pig on his shoulders, pursued by the proprietor with a long gun pointed at him, kept dodging round me to save himself; I bid him throw down the pig and run, which he did; and the proprietor, satisfied with having recovered his loss, acquainted me with my danger, by telling me that there were now thieves in every house that was left standing. I thought it therefore high time to retire, both for my own safety, and that I might endeavour to procure from Naples some protection for the doubly unfortunate sufferers of this unhappy town. Accordingly I returned to Naples in my boat, and immediately acquainted this government with what I had just seen myself;

in consequence of which a body of soldiers was sent directly to their relief by sea, the road by land having been cut off by the lava. I remarked in my way home, that there was a much greater quantity of the petroleum floating on the surface of the sea, and diffusing a very strong and offensive smell, than was usual; for at all times in calms, patches of this bituminous oil, called here petroleum, are to be seen floating on the surface of the sea between Portici and Naples, and particularly opposite a village called Pietra Bianca. The minute ashes continued falling all this day at Naples; the mountain, totally obscured by them, continued to alarm us with repeated loud explosions; the streets of this city were this day and the next constantly filled with religious and penitential processions, composed of all classes, and nothing was heard in the midst of darkness but the thunder of the mountain, and *ora pro nobis*. The sea wind increasing at times, delivered us from these ashes, which it scattered over different parts of the Campagna Felice.

On Wednesday the 18th, the wind having for a very short space of time cleared away the thick cloud from the top of Vesuvius, we discovered that a great part of its crater, particularly on the west side opposite Naples, had fallen in, which it probably did about four o'clock in the morning of this day, as a violent shock of an earthquake was felt at that moment at Resina, and other parts situated at the foot of the volcano. The clouds of smoke, mixed with the ashes which, as I have before remarked, were as fine as Spanish snuff (so much so that the impression of a seal with my coat of arms would remain distinctly marked upon them), were of such a density as to appear to have the greatest difficulty in forcing their passage out of the now widely extended mouth of Vesuvius, which

certainly, since the top fell in, cannot be much short of two miles in circumference. One cloud heaped on another, and succeeding one another incessantly, formed in a few hours such a gigantic and elevated column of the darkest hue over the mountain, as seemed to threaten Naples with immediate destruction, having at one time been bent over the city, and appearing to be much too massive and ponderous to remain long suspended in the air; it was besides replete with the *ferilli*, or volcanic lightning, which was stronger than common lightning, just as PLINY the younger describes it in one of his letters to TACITUS, when he says *fulgoribus illæ et similes et majores erant.*

Vesuvius was at this time completely covered, as were all the old black lavas, with a thick coat of these fine light-grey ashes already fallen, which gave it a cold and horrid appearance; and in comparison of the abovementioned enormous mass of clouds, which certainly, however it may contradict our idea of the extension of our atmosphere, rose many miles above the mountain, it appeared like a mole-hill; although, as you know, Sir, the perpendicular height of Vesuvius from the level of the sea, is more than three thousand six hundred feet. The Abbé BRACCINI, as appears in his printed account of the eruption of Mount Vesuvius in 1631, measured with a quadrant the elevation of a mass of clouds of the same nature, that was formed over Vesuvius during that great eruption, and found it to exceed thirty miles in height. Doctor SCOTTI, in his printed account of this eruption, says that the height of this threatening cloud of smoke and ashes, measured (but he does not say how) from Naples, was found to be of an elevation of thirty degrees. All I can say is, that to my eye

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the distance from the crater of Vesuvius to the most elevated part of the cloud, appeared to me nearly the same as that of the island of Caprea from Naples, and which is about 25 miles; but I am well aware of the inaccuracy of such a sort of measurement. At the time of its greatest elevation, I engaged Signor Gatta, successor to the late ingenious Mr. Fabris, to make an exact drawing of it, which he did with great success; and a copy of that drawing on a small scale is inclosed (Tab. VII.), and will, I hope, give you a very good idea of what I have been describing.

I must own, that at that moment I did apprehend Naples to be in some danger of being buried under the ashes of the volcano, just as the towns of Herculaneum and Pompeii were in the year 79. The ashes that fell then at Pompeii were of the same fine quality as those from this eruption; having often observed, when present at the excavations of that ancient city, that the ashes, which I suppose to have been mixed with water at the same time, had taken the exact impression or mould of whatever they had inclosed; so that the compartments of the wood work of the windows and doors of the houses remained impressed on this volcanic tufo, although the wood itself had long decayed, and not an atom of it was to be seen, except when the wood had been burnt, and then you found the charcoal. Having once been present at the discovery of a skeleton in the great street of Pompeii, of a person who had been shut up by the ashes during the eruption of 79, I engaged the men that were digging to take off the piece of hardened tufo, that covered the head, with great care, and, as in a mould just taken off in plaster of Paris, we found the impression of the eyes, that were shut, of the

nose, mouth, and of every feature perfectly distinct. A similar specimen of a mould of this kind, brought from Pompeii, is now in his Sicilian Majesty's museum at Portici; it had been formed over the breast of a young woman that had been shut up in the volcanic matter; every fold of a thin drapery that covered her breast is exactly represented in this mould: and in the volcanic tufo that filled the ancient theatre of Herculaneum, the exact mould or impression of the face of a marble bust is still to be seen, the bust or statue having been long since removed. Having observed these fine ashes issuing in such abundance from Vesuvius, and having the appearance of being damp or wet, as you may perceive by the drawing (Tab. VII.) that they do not take such beautiful forms and volutes as a fine dry smoke usually does, but appear in harsh and stiff little curls, you will not wonder then, that the fate of Herculaneum and Pompeii should have come again strongly into my mind; but fortunately the wind sprung up fresh from the sea, and the threatening cloud bent gradually from us over the mountain of Somma, and involved all that part of the Campagna in obscurity and danger.

To avoid prolixity and repetition, I need only say, that the storms of thunder and lightning, attended at times with heavy falls of rain and ashes, causing the most destructive torrents of water and glutinous mud, mixed with huge stones, and trees torn up by the roots, continued more or less to afflict the inhabitants on both sides of the volcano until the 7th of July, when the last torrent destroyed many hundred acres of cultivated land, between the towns of Torre del Greco and Torre dell' Annunziata. Some of these torrents, as I have been credibly assured by eye witnesses, both on the

sea side and the Somma side of the mountain, came down with a horrid rushing noise; and some of them, after having forced their way through the narrow gullies of the mountain, rose to the height of more than 20 feet, and were near half a mile in extent. The mud of which the torrents were composed, being a kind of natural mortar, has completely cased up, and ruined for the present, some thousand acres of rich vineyards; for it soon becomes so hard, that nothing less than a pick-axe can break it up; I say for the present, as I imagine that hereafter the soil may be greatly improved by the quantity of saline particles that the ashes from this eruption evidently contain. A gentleman of the British factory at Naples, having filled a plate with the ashes that had fallen on his balcony during the eruption, and sowed some pease in them, assured me that they came up the third day, and that they continue to grow much faster than is usual in the best common garden soil.

My curiosity, or rather my wish to gratify that of our respectable Society, induced me to go upon Mount Vesuvius, as soon as I thought I might do it with any degree of prudence, which was not until the 30th of June, and then it was attended with some risk, as will appear in the course of this narrative. The crater of Vesuvius, except at short intervals, had been continually obscured by the volcanic clouds ever since the 16th, and was so this day, with frequent flashes of lightning playing in those clouds, and attended as usual with a noise like thunder; and the fine ashes were still falling on Vesuvius, but still more on the mountain of Somma. I went up the usual way by Resina, attended by my old Cicerone of the mountain, BARTOLOMEO PUMO, with whom I have been

sixty-eight times on the highest point of Vesuvius. I observed in my way through the village of Resina that many of the stones of the pavement had been loosened, and were deranged by the earthquakes, particularly by that of the 18th, which attended the falling in of the crater of the volcano, and which, as they told me there, had been so violent as to throw many people down, and obliged all the inhabitants of Resina to quit their houses hastily, and to which they did not dare return for two days. The leaves of all the vines were burnt by the ashes that had fallen on them, and many of the vines themselves were buried under the ashes, and great branches of the trees that supported them had been torn off by their weight. In short, nothing but ruin and desolation was to be seen. The ashes at the foot of the mountain were about 10 or 12 inches thick on the surface of the earth, but in proportion as we ascended their thickness increased to several feet, I dare say not less than 9 or 10 in some parts; so that the surface of the old rugged lavas, that before was almost impracticable, was now become a perfect plain, over which we walked with the greatest ease. The ashes were of a light-grey colour, and exceedingly fine, so that by the footsteps being marked on them as on snow, we learnt that three small parties had been up before us. We saw likewise the track of a fox, that appeared to have been quite bewildered, to judge from the many turns he had made. Even the traces of lizards and other little animals, and of insects, were visible on these fine ashes. We ascended to the spot from whence the lava of the 15th first issued, and we followed the course of it, which was still very hot (although covered with such a thick coat of ashes), quite down to the sea at Torre del Greco, which is more than five miles. A pair

of boots, to which I had for the purpose added a new and thick sole, were burnt through on this expedition. It was not possible to get up to the great crater of Vesuvius, nor had any one yet attempted it. The horrid chasms that exist from the spot where the late eruption first took place, in a straight line for near two miles towards the sea, cannot be imagined. They formed vallies more than two hundred feet deep, and from half to a mile wide; and where the fountains of fiery matter existed during the eruption, are little mountains with deep craters. Ten thousand men, in as many years, could not, surely, make such an alteration on the face of Vesuvius, as has been made by nature in the short space of five hours. Except the exhalations of sulphureous and vitriolic vapours, which broke out from different spots of the line abovementioned, and tinged the surface of the ashes and scoriæ in those parts with either a deep or pale yellow with a reddish ochre colour, or a bright white, and in some parts with a deep green and azure blue (so that the whole together had the effect of an iris), all around us had the appearance of a sandy desert. We went on the top of seven of the most considerable of the new-formed mountains, and looked into their craters, which on some of them appeared to be little short of half a mile in circumference; and although the exterior perpendicular height of any of them did not exceed two hundred feet, the depth of their inverted cone within was three times as great. It would not have been possible for us to have breathed on these new mountains near their craters, if we had not taken the precaution of tying a doubled handkerchief over our mouths and nostrils; and even with that precaution we could not resist long, the fumes of the vitriolic acid were so exceedingly penetrating, and of

such a suffocating quality. We found in one a double crater, like two funnels joined together; and in all there was some little smoke and depositions of salts and sulphurs, of the various colours above mentioned, just as is commonly seen adhering to the inner walls of the principal crater of Vesuvius.

Two or three days after we had been here, one of the new mouths into which we had looked, suddenly made a great explosion of stones, smoke, and ashes, which would certainly have proved fatal to any one who might unfortunately have been there at the time of the explosion. We read of a like accident having proved fatal to more than twenty people, who had the curiosity to look into the crater of the Monte Nuovo, near Pozzuoli, a few days after its formation, in the year 1538. The 15th of August, I saw a sudden explosion of smoke and ashes, thrown to an extreme height out of the great crater of Vesuvius, that must have destroyed any one within half a mile of it; and yet on the 19th of July a party not only had visited that crater, but had descended 170 feet within it. Whilst we were on the mountain, two whirlwinds, exactly like those that form water-spouts at sea, made their appearance; and one of them that was very near us made a strange rushing noise, and having taken up a great quantity of the fine ashes, formed them into an elevated spiral column, which, with a whirling motion and great rapidity, was carried towards the mountain of Somma, where it broke and was dispersed. As there were evident signs of an abundance of electricity in the air at this time, I have no doubt of this having been also an electrical operation. One of my servants, employed in collecting of sulphur, or sal ammoniac, which crystallizes near the *fumaroli*, as they are called here (and which are the spots from whence

the hot vapour issues out of the fresh lavas), found to his great surprise an exceeding cold wind issue from a fissure very near the hot *fumaroli* abovementioned upon his leg; I put my hand to the spot, and found the same; but it did not surprise me, as before on Mount Vesuvius, on the mountain of Somma, on Mount Etna, and in the island of Ischia, I had met with, on particular spots, the like currents of extreme cold air issuing from beneath the ancient lavas, and which, being constant to those spots, are known by the name of *ventoroli*. In a vineyard not in the same line with the new-formed mountains just described, but in a right line from them, at the distance of little more than a mile from Torre del Greco, are three or four more of these new-formed mountains with craters, out of which the lava flowed, and by uniting with the streams that came from the higher mouths, and adding to their heat and fluidity, enabled the whole current to make so rapid a progress over the unfortunate town, as scarcely to allow its inhabitants sufficient time to escape with their lives. The rich vineyards belonging to the Torre del Greco, and which produced the good wine called *Lacrima Christi*, that have been buried, and are totally destroyed by this lava, consisted, as I have been informed, of more than three thousand acres; but the destruction of the vineyards by the torrents of mud and water at the foot of the mountain of Somma, is much more extensive.

I visited that part of the country also a few days after I had been on Vesuvius, not being willing to relate to you any one circumstance of the late formidable eruption but what I had reason to believe was founded on truth. The first signs of a torrent that I met with, was near the village of the Madonna

dell' Arco, and I passed several others between that and the town of Ottaiano; the one near Trochia, and two near the town of Somma, were the most considerable, and not less than a quarter of a mile in breadth; and as several eye witnesses assured me on the spot, were, when they poured down from the mountain of Somma, from 20 to 30 feet high; it was a liquid glutinous mud, composed of scoriæ, ashes, stones (some of which of an enormous size) mixed with trees that had been torn up by the roots. Such torrents, as you may well imagine, were irresistible, and carried all before them; houses, walls, trees, and, as they told me, not less than four thousand sheep and other cattle, had been swept off by the several torrents on that side of the mountain. At Somma they likewise told me that a team of eight oxen, that were drawing a large timber tree, had been carried off from thence, and never were more heard of.

The appearance of these torrents, when I saw them, was like that of all other torrents in mountainous countries, except that what had been mud was become a perfect cement, on which nothing less than a pick-axe could make any impression. The vineyards and cultivated lands were here much more ruined; and the limbs of the trees much more torn by the weight of the ashes, than those which I have already described on the sea side of the volcano.

The Abbé Tata, in his printed account of this eruption, has given a good idea of the abundance, the great weight, and glutinous quality of these ashes, when he says that having taken a branch from a fig-tree still standing near the town of Somma, on which were only six leaves, and two little unripe figs, and having weighed it with the ashes attached to it, he

found it to be 31 ounces; when having washed off the volcanic matter, it scarcely weighed 3 ounces.

I saw several houses on the road, in my way to the town of Somma, with their roofs beaten in by the weight of the ashes. In the town of Somma, I found four churches and about seventy houses without roofs, and full of ashes. The great damage on this side of the mountain, by the fall of the ashes and the torrents, happened on the 18th, 19th, and 20th of June, and on the 12th of July. I heard but of three lives that had been lost at Somma by the fall of a house. The 19th, the ashes fell so thick at Somma (as they told me there), that unless a person kept in motion, he was soon fixed to the ground by them. This fall of ashes was accompanied also with loud reports, and frequent flashes of the volcanic lightning, so that, surrounded by so many horrors, it was impossible for the inhabitants to remain in the town, and they all fled; the darkness was such, although it was mid-day, that even with the help of torches it was scarcely possible to keep in the high road; in short, what they described to me was exactly what PLINY the younger and his mother had experienced at Misenum during the eruption of Vesuvius in the reign of TITUS, according to his second letter to TACITUS on that subject. I found that the majority of people here were convinced that the torrents of mud and water, that had done them so much mischief, came out of the crater of Vesuvius, and that it was sea-water; but there cannot be any doubt of those floods having been occasioned by the sudden dissolution of watery clouds mixed with ashes, the air perhaps having been too much rarefied to support them; and when such clouds broke, and fell heavily on Vesuvius, the water not being able to penetrate

as usual into the pores of the earth, which were then filled up with the fine ashes of a bituminous and oily quality, nor having free access to the channels which usually carried it off, accumulated in pools, and mixing with more ashes, rose to a great height, and at length forced its way through new channels, and came down in torrents over countries where it was least expected, and spread itself over the fertile lands at the foot of the mountain. From what I have seen lately, I begin to doubt very much if the water, by which so much damage was done, and so many lives were lost during the terrible eruption of Vesuvius in 1631, did really, as was generally supposed, come out of the crater of the volcano: sentiments were divided then, as they are now, on that subject; and since in all great eruptions the crater of the volcano must be obscured by the clouds of ashes, as it probably was then, and certainly was during the violence of the late eruption, therefore it must be very difficult to ascertain exactly from whence that water came. The more extraordinary a circumstance is, the more it appears to be the common desire that it should be credited; from this principle, one of his Sicilian Majesty's gardeners of Portici went up to the crater of Vesuvius as soon as it was practicable, and came down in a great fright, declaring that he had seen it full of boiling water. The Chevalier MACEDONIO, intendant of Portici, judged very properly, that to put an end to the alarm this report had spread over the country, it was necessary to send up people he could trust, and on whose veracity he might depend. Accordingly the next day, which was the 16th of July, Signor GUISEPPE SACCO went up, well attended, and proved the gardener's assertion to be absolutely false, there being only some little signs of mud from a deposition of the

rain water at the bottom of the crater. According to SACCO'S account, which has been printed at Naples, the crater is of an irregular oval form, and, as he supposes (not having been able to measure it) of about a mile and an half in circumference; by my eye I should judge it to be more; the inside, as usual, in the shape of an inverted cone, the inner walls of which on the eastern side are perpendicular; but on the western side of the crater, which is much lower, the descent was practicable, and SACCO with some of his companions actually went down 176 palms, from which spot, having lowered a cord with a stone tied to it, they found the whole depth of the crater to be about 500 palms. But such observations on the crater of Vesuvius are of little consequence, as both its form and apparent depth are subject to great alterations from day to day. These curious observers certainly ran some risk at that time, since which such a quantity of scoriæ and ashes have been thrown up from the crater, and even so lately as the 15th of this month, as must have proved fatal to any one within their reach.

The 22d of July, one of the new craters, which is the nearest to the town of Torre del Greco, threw up both fire and smoke, which circumstance, added to that of the lava's retaining its heat much longer than usual, seems to indicate that there may still be some fermentation under that part of the volcano. The lava in cooling often cracks, and causes a loud explosion, just as the ice does in the Glaciers in Switzerland; such reports are frequently heard now at the Torre del Greco; and as some of the inhabitants told me, they often see a vapour issue from the body of the lava, and taking fire in air, fall like those meteors vulgarly called falling stars.

The darkness occasioned by the fall of the ashes in the Campagna Felice extended itself, and varied, according to the prevailing winds. On the 19th of June it was so dark at Caserta, which is 15 miles from Naples, as to oblige the inhabitants to light candles at mid-day; and one day during the eruption, the darkness spread over Beneventum, which is 30 miles from Vesuvius.

The Archbishop of Taranto, in a letter to Naples, and dated from that city the 18th of June, said, "We are involved in a "thick cloud of minute volcanic ashes, and we imagine that "there must be a great eruption either of Mount Etna, or of "Stromboli." The bishop did not dream of their having proceeded from Vesuvius, which is about 250 miles from Taranto. We have had accounts also of the fall of the ashes during the late eruption at the very extremity of the province of Lecce, which is still farther off; and we have been assured likewise, that those clouds were replete with electrical matter: at Martino, near Taranto, a house was struck and much damaged by the lightning from one of these clouds. In the accounts of the great eruption of Vesuvius in 1631, mention is made of the extensive progress of the ashes from Vesuvius, and of the damage done by the *ferilli*, or volcanic lightning, which attended them in their course.

I must here mention a very extraordinary circumstance indeed, that happened near Sienna in the Tuscan state, about 18 hours after the commencement of the late eruption of Vesuvius on the 15th of June, although that phænomenon may have no relation to the eruption; and which was communicated to me in the following words by the Earl of Bristol, bishop of Derry, in a letter dated from Sienna, July 12th, 1794: "In

" the midst of a most violent thunder-storm, about a dozen " stones of various weights and dimensions fell at the feet of " different people, men, women, and children; the stones are " of a quality not found in any part of the Siennese territory; " they fell about 18 hours after the enormous eruption of Ve- " suvius, which circumstance leaves a choice of difficulties in " the solution of this extraordinary phænomenon: either these " stones have been generated in this igneous mass of clouds, " which produced such unusual thunder, or, which is equally " incredible, they were thrown from Vesuvius at a distance of " at least 250 miles; judge then of its parabola. The philoso- " phers here incline to the first solution. I wish much, Sir, to " know your sentiments. My first objection was to the fact " itself; but of this there are so many eye witnesses, it seems " impossible to withstand their evidence, and now I am re- " duced to a perfect scepticism." His lordship was pleased to send me a piece of one of the largest stones, which when entire weighed upwards of five pounds; I have seen another that has been sent to Naples entire, and weighs about one pound. The outside of every stone that has been found, and has been ascertained to have fallen from the cloud near Sienna, is evidently freshly vitrified, and is black, having every sign of having passed through an extreme heat; when broken, the inside is of a light-grey colour mixed with black spots, and some shining particles, which the learned here have decided to be pyrites, and therefore it cannot be a lava, or they would have been decomposed. Stones of the same nature, at least as far as the eye can judge of them, are frequently found on Mount Vesuvius; and when I was on the mountain lately, I searched for such stones near the new mouths, but as the soil

round them has been covered with a thick bed of fine ashes, whatever was thrown up during the force of the eruption lies buried under those ashes. Should we find similar stones with the same vitrified coat on them on Mount Vesuvius, as I told Lord BRISTOL in my answer to his letter, the question would be decided in favour of Vesuvius; unless it could be proved that there had been, about the time of the fall of these stones in the Sanese territory, some nearer opening of the earth, attended with an emission of volcanic matter, which might very well be, as the mountain of Radicofani, within 50 miles of Sienna, is certainly volcanic. I mentioned to his lordship another idea that struck me. As we have proofs during the late eruption of a quantity of ashes of Vesuvius having been carried to a greater distance than where the stones fell in the Sanese territory, might not the same ashes have been carried over the Sanese territory, and mixing with a stormy cloud, have been collected together just as hailstones are sometimes into lumps of ice, in which shape they fall; and might not the exterior vitrification of those lumps of accumulated and hardened volcanic matter have been occasioned by the action of the electric fluid on them? The celebrated Father AMBROGIO SOLDANI, professor of mathematics in the university of Sienna, is printing there his dissertation upon this extraordinary phænomenon; wherein, as I have been assured, he has decided that those stones were generated in the air independantly of volcanic assistance.

Until after the 7th of July, when the last cloud broke over Vesuvius, and formed a tremendous torrent of mud, which took its course across the great road between Torre del Greco and the Torre dell' Annunziata, and destroyed many vineyards,

the late eruption could not be said to have finished, although the force of it was over the 22d of June, since which time the crater has been usually visible. The power of attraction in mountains is well known; but whether the attractive power of a volcanic mountain be greater than that of any other mountain, is a question: all I can say is, that during this last eruption every watery cloud has been evidently attracted by Vesuvius, and the sudden dissolution of those clouds has left such marks of their destructive power on the face of the country all round the basis of the volcano as will not soon be erased. Since the mouth of Vesuvius has been enlarged, I have seen a great cloud passing over it, and which not only was attracted, but was sucked in, and disappeared in a moment.

After every violent eruption of Mount Vesuvius, we read of damage done by a mephitic vapour, which coming from under the ancient lavas, insinuates itself into low places, such as the cellars and wells of the houses situated at the foot of the volcano. After the eruption of 1767, I remember that there were several instances, as in this, of people going into their cellars at Portici, and other parts of that neighbourhood, having been struck down by this vapour, and who would have expired if they had not been hastily removed. These occasional vapours, and which are called here *mofete*, are of the same quality as that permanent one in the Grotta del Cane, near the lake of Agnano, and which has been proved to be chiefly fixed air. The vapours, that in the volcanic language of this country are called *fumaroli*, are of another nature, and issue from spots all over the fresh and hot lavas whilst they are cooling; they are sulphureous and suffocating, so much so that often the birds that are flying over them are overpowered, and fall down dead; of

which we have had many examples during this eruption, particularly of wood pigeons, that have been found dead on the lava. These vapours deposite a crust of sulphur, or salts, particularly of sal ammoniac, on the scoriæ of the lava through which they pass; and the small crystals of which they are composed are often tinged with a deep or pale yellow, with a bright red like cinnabar, and sometimes with green, or an azure blue. Since the late eruption, many pieces of the scoriæ of the fresh lava have been found powdered with a lucid substance, exactly like the brightest steel or iron filings.

The first appearance of the *mofete*, after the late eruption, was on the 17th of June, when a peasant going with an ass to his vineyard, a little above the village of Resina, in a narrow hollow way, the ass dropped down, and seemed to be expiring; the peasant was soon sensible of the mephitic vapour himself, and well knowing its fatal effects, dragged the animal out of its influence, and it soon recovered. From that time these vapours have greatly increased, and extended themselves. There are to this day many cellars and wells, all the way from Portici to Torre dell' Annunziata, greatly affected by them. This heavy vapour, when exposed to the open air, does not rise much more than a foot above the surface of the earth, but when it gets into a confined place, like a cellar or well, it rises and fills them as any other fluid would do; having filled a well, it rises above it about a foot high, and then bending over, falls to the earth, on which it spreads, always preserving its usual level. Wherever this vapour issues, a wavering in the air is perceptible, like that which is produced by the burning of charcoal; and when it issues from a fissure near any plants or vegetables, the leaves of those plants are seen to move, as if

they were agitated by a gentle wind. It is extraordinary, that although there does not appear to be any poisonous quality in this vapour, which in every respect resembles fixed air, it should prove so very fatal to the vineyards, some thousand acres of which have been destroyed by it since the late eruption; when it penetrates to the roots of the vines, it dries them up, and kills the plant. A peasant in the neighbourhood of Resina having suffered by the *mofete*, which destroyed his vineyards in the year 1767, and having observed then that the vapour followed the laws of all fluids, made a narrow deep ditch all round his vineyard, which communicated with ancient lavas, and also to a deep cavern under one of them; the consequence of his well reasoned operation has been, that although surrounded at present by these noxious vapours, and which lie constantly at the bottom of his ditch, they have never entered his vineyard, and his vines are now in a flourishing state, whilst those of his neighbours are perishing. Upwards of thirteen hundred hares, and many pheasants and partridges, overtaken by this vapour, have been found dead within his Sicilian Majesty's reserved chases in the neighbourhood of Vesuvius; and also many domestic cats, who in their pursuit after this game fell victims to the *mofete*. A few days ago a shoal of fish, of several hundred weight, having been observed by some fishermen at Resina in great agitation on the surface of the sea, near some rocks of an ancient lava that had run into the sea, they surrounded them with their nets, and took them all with ease, and afterwards discovered that they had been stunned by the mephitic vapour, which at that time issued forcibly from underneath the ancient lava into the sea. I have been assured by many fishermen,

that during the force of the late eruption the fish had totally abandoned the coast from Portici to the Torre dell' Annunziata, and that they could not take one in their nets nearer the shore than two miles. The divers there, who fish for the *ancini* (which we call sea eggs) and other shell fish, likewise told me, that for the space of a mile from that shore, since the eruption, they have found all the fish dead in their shells, as they suppose either from the heat of the sand at the bottom of the sea, or from poisonous vapours. The divers at Naples complain of their finding also many of these shell fish, or as they are called here in general terms, *frutti di mare,* dead in their shells.

I thought that these little well attested facts might contribute to show the great force of the wonderful chemical operation of nature that has lately been exhibited here. The *mofete,* or fixed air vapours, must certainly have been generated by the action of the vitriolic acid upon the calcareous earth, as both abound in Vesuvius. The sublimations, which are visibly operating by the chemistry of nature all along the course of the last lava that ran from Vesuvius, and particularly in and about the new mouths that have been formed by the late eruption on the flanks of the volcano, having been analyzed by Signor DOMENICO TOMASO, an ingenious chemist of Naples, and whose experiments, and the result of them, are now published, have been found to be chiefly sal ammoniac, mixed with a small quantity of the calx of iron: but not to betray my ignorance on this subject, and pretending to nothing more than the being an exact ocular observer, I refer you to the work itself, which accompanies this letter. Many hundred weight of the Vesuvian sal ammoniac have been collected on the mountain since the late eruption by the peasants, and sold at

Naples to the refiners of metals; at first it was sold for about six pence a pound, but, from its abundance, the price is now reduced to half that money; and a much greater quantity must have escaped in the air by evaporation.

The situation of Mount Vesuvius so near a great capital, and the facility of approaching it, has certainly afforded more opportunities of watching the operations of an active volcano, and of making observations upon it, than any other volcano on the face of the earth has allowed of. The Vesuvian diary, which by my care has now been kept with great exactness, and without interruption for more than 15 years, by the worthy and ingenious Padre ANTONIO PIAGGI, as mentioned in the beginning of this letter, and which it is my intention to deposite in the library of the Royal Society, will also throw a great light upon this curious subject. But as there is every reason to believe, with SENECA,* that the seat of the fire that causes these eruptions of volcanoes is by no means superficial, but lies deep in the bowels of the earth, and where no eye can penetrate, it will, I fear, be ever much beyond the reach of the limited human understanding to account for them with any degree of accuracy. There are modern philosophers who propose, with as great confidence, the erecting of conductors to prevent the bad effects of earthquakes and volcanoes, and who promise themselves the same success as that which has attended Doctor FRANKLIN's conductors of lightning; for, as they say, all proceed from one and the same cause, *electricity*. When we reflect how many parts of the earth already inhabited have evidently been thrown up from the bottom of the

* "Non ipse ex se est, sed in aliqua inferna valle conceptus exæstuat, et alibi pas-"citur; *in ipso monte non alimentum babet, sed viam.*"—SENECA, Epist. 79.

sea by volcanic explosions, and the probability of there being a much greater portion under the same predicament, as yet unexplored, the vain pretensions of weak mortals to counteract such great operations, carried on surely for the wisest purposes by the beneficent Author of nature, appear to me to be quite ridiculous.

Let us then content ourselves with seeing, as well as we can, what we are permitted to see, and reason upon it to the best of our limited understandings, well assured that whatever is, is right.

The late sufferers at Torre del Greco, although his Sicilian Majesty, with his usual clemency, offered them a more secure spot to rebuild their town on, are obstinately employed in rebuilding it on the late and still smoking lava that covers their former habitations; and there does not appear to be any situation more exposed to the numerous dangers that must attend the neighbourhood of an active volcano than that of Torre del Greco. It was totally destroyed in 1631; and in the year 1737 a dreadful lava ran within a few yards of one of the gates of the town, and now over the middle of it; nevertheless, such is the attachment of the inhabitants to their native spot, although attended with such imminent danger, that of 18000 not one gave his vote to abandon it. When I was in Calabria, during the earthquakes in 1783, I observed in the Calabrese the same attachment to native soil; some of the towns that were totally destroyed by the earthquakes, and which had been ill situated in every respect, and in a bad air, were to be rebuilt; and yet it required the authority of government to oblige the inhabitants of those ruined towns to change their situation for a much better.

Upon the whole, having read every account of the former eruptions of Mount Vesuvius, I am well convinced that this eruption was by far the most violent that has been recorded after the two great eruptions of 79 and 1631, which were undoubtedly still more violent and destructive. The same phænomena attended the last eruption as the two former above mentioned, but on a less scale, and without the circumstance of the sea having retired from the coast. I remarked more than once, whilst I was in my boat, an unusual motion in the sea during the late eruption. On the 18th of June I observed, and so did my boatman, that although it was a perfect calm, the waves suddenly rose and dashed against the shore, causing a white foam, but which subsided in a few minutes. On the 15th, the night of the great eruption, the corks that support the nets of the royal tunny fishery at Portici, and which usually float upon the surface of the sea, were suddenly drawn under water, and remained so for a short space of time, which indicates, that either there must have been at that time a swell in the sea, or a depression or sinking of the earth under it.

From what we have seen lately here, and from what we read of former eruptions of Vesuvius, and of other active volcanoes, their neighbourhood must always be attended with danger; with this consideration, the very numerous population at the foot of Vesuvius is remarkable. From Naples to Castel-a-mare, about 15 miles, is so thickly spread with houses as to be nearly one continued street, and on the Somma side of the volcano, the towns and villages are scarcely a mile from one another; so that for thirty miles, which is the extent of the basis of Mount Vesuvius and Somma, the population may be perhaps more numerous than that of any spot of a like

extent in Europe, in spite of the variety of dangers attending such a situation.

With the help of the drawings that accompany this account of the late eruption of Vesuvius, and which I can assure you to be faithful representations of what we have seen, I flatter myself I shall have enabled you to have a clear idea of it; and I flatter myself also, that the communication of such a variety of well attested phænomena as have attended this formidable eruption, may not only prove acceptable, but useful to the curious in natural history.

I have the honour to be, &c.

WM. HAMILTON.

IN a subsequent letter from Sir WILLIAM HAMILTON to Sir JOSEPH BANKS, dated *Castel-a-mare,* anciently *Stabiæ,* Sept. 2, 1794, are the two following remarks to be added to this paper.

1. Within a mile of this place the *mofete* are still very active, and particularly under the spot where the ancient town of Stabiæ was situated. The 24th of August, a young lad by accident falling into a well there that was dry, but full of the mephitic vapour, was immediately suffocated; there were no signs of any hurt from the fall, as the well was shallow. This circumstance called to my mind the death of the elder PLINY, who most probably lost his life by the same sort of mephitic vapours, on this very spot, and which are active after great eruptions of Vesuvius.

2. Mr. JAMES, a British merchant, who now lives in this neighbourhood, assured me that on Tuesday night, the 17th of

June, which was the third day of the eruption of Mount Vesuvius, he was in a boat with a sail, near Torre del Greco, when the minute ashes, so often mentioned in my letter, fell thick; and that in the dark they emitted a pale light like phosphorus, so that his hat, those of the boatmen, and the part of the sails that were covered with the ashes, were luminous. Others have mentioned to me the having seen a phosphoric light on Vesuvius after this eruption; but until it was confirmed to me by Mr. JAMES, I did not choose to say any thing about it.

EXPLANATION OF THE PLATES.

Tab. V. Is a view of the eruption of Mount Vesuvius on the night of the 15th of June, 1794, taken from S. Lucia at Naples, when the eruption was in its greatest force.

Tab. VI. Is a view of the lava that destroyed the town of Torre del Greco, taken from a boat on the sea near that town, about five o'clock in the morning of the 16th of June, and whilst the lava was still advancing in the sea. The rocks, on which are two figures near the boat, were formed by a lava that ran into the sea during a former eruption of Mount Vesuvius.

Tab. VII. Is a view of the enormous cloud of smoke and ashes, replete with *ferilli*, or volcanic lightning, which first threatened destruction to the town of Naples on the 18th of June; and afterwards, from the impulse of the sea wind, bent over the mountain of Somma, and poured its destructive contents on the towns situated at the foot of that mountain, beating in the roofs of the houses, and involving all the inhabitants of the Campagna Felice in darkness and danger. This

view was taken from Naples, and gives a very good idea of the appearance of Mount Vesuvius, like a mole-hill, in comparison of the enormous mass that hung over it.

Tab. VIII. Is a view of Mount Vesuvius, and of Somma, taken from Posilipo July 6th, 1794, when it could be clearly distinguished; the dotted lines shew the form of the top of Vesuvius as it was before this eruption, and when the crater was only from A to B; the present wide extended crater is sufficiently plain in the drawing not to need any further explanation; the spot from whence the lava first issued the night of the 15th of June, is marked C.

These four very exact drawings were taken from nature by Signor XAVERIO GATTA, successor to Signor PIETRO FABRIS.

Tab. IX. Is a drawing made by the Padre ANTONIO PIAGGI at Resina, during the force of the eruption of the 15th at night; and being within a mile and a half of the mountain, shews many particulars that escaped us, so much farther off at Naples; but he was interrupted by the imminent danger of his situation, and his drawing is incomplete: it was with difficulty that his friends carried him off alive, being upwards of 80 years old, in the midst of a shower of heavy cinders and sulphureous ashes, an hour after the beginning of the eruption; nor was he able to return to his house for many days. Nothing is necessary to be added to his Latin references to the drawing, but that Turris VIII. is *Torre del Greco*, and Retina, now *Resina*.

A. Montis vertex innubis, compositusque.

B. ad H. Sulci rudes inhianti terræ frequenter inscripti.

D. Ignei rivi fluentes Retinam versus.

E. Nitidissima flamma in cupressus formam altitudinem montis exsuperans.

F. Saxorum tempestas in altum a voraginibus erumpentium.

G. Lenis clivus igneum flumen in Retinam minantem avertens.

H. Semita ignei torrentis incredibili rapiditate Turrim VIII. invasuri.

I. Arbores, et vineta simplici illius afflatu a longe micantia.

K. Turris VIII. quæ Herculanio successisse creditur.

L. S^ta. Mariæ Apulianæ templum.

M. Retina templo adhærens, recenter constructa, ab illo usque ad mare.

N. Porticus: nova item constructio Neapolim versus, unum corpus cum Retina efficiens.

O. Leucopetra.

P. Massa.

Q. Trochlea.

R. S^ti. Sebastiani vicus.

S. Fumus lapillis, asperis arenis, et aqua marina confertus in pluviam solutus.

Tab. X. Plan of the city of Torre del Greco, destroyed in great part by the lava which ran in the night of the 15th of June, 1794.

Tab. XI. Map of Mount Vesuvius and the adjacent places, with the course of the lava.

V. *New Observations in further Proof of the mountainous Inequalities, Rotation, Atmosphere, and Twilight, of the Planet Venus. By* John Jerome Schroeter, *Esq. Communicated by* George Best, *Esq. F. R. S.*

(Translated from the German.)

Read February 19, 1795.

PREFACE.

ALTHOUGH it is a satisfaction to me, that Dr. HERSCHEL last year found my discovery of the morning and evening twilight of Venus's atmosphere to be confirmed, as I could not hope to have obtained such an important confirmation so early, considering the excellent telescopes required, and that a favourable opportunity for such observations occurs but rarely; yet the paper *on the Planet Venus,* which this great observer has inserted in the Philosophical Transactions for 1793, contains unreserved assertions, which may be easily injurious to the truth, for the very reason that they have truth for their object, and yet rest on no sufficient foundation.

Openness, without reserve or indirect views, must guide the spirit of observation in the true inquirer into nature, and be his sole object. To this pure source alone can I ascribe what is said in the abovementioned paper, so as to reconcile it to the friendly sentiments which the author has always hitherto expressed toward me, and which I hold extremely precious;

though perhaps to others it may not have the same appearance. But this very object makes it also my duty to be equally unreserved in remarking what truth is, and demands; particularly as evident misunderstanding and error appear to have chiefly occasioned those assertions; which most probably would not have been thus made, if the author had then known of my very circumstantial memoir,* which was read at the jubilee of the university of Erfurt, in a meeting of the Electoral Academy of Sciences, and which they ordered to be printed; and could have compared the many careful observations, full of matter, contained in it. A copy of this memoir I have lately had the honour of communicating to the worthy author of the abovementioned paper.

Therefore, in order to prevent misapprehensions, let me be allowed to make some remarks, which truth requires of me, before I communicate faithfully, as I mean to do, my more recent observations, which confirm the former ones, and seem to me very important.

1. The celebrated author considers it, *with reason*, as a wonderful relation, that I should profess to have seen *appearances of spherical spots* on Saturn, without having, at the same time, determined from them the period of his rotation, which might have been done in the first hour; and he thinks that no one, who is not possessed of incomparably better sight and telescopes than he has, can have seen any thing of the kind. In that I fully agree with him, and here declare publicly, *that I have never perceived such an appearance on Saturn, however much I wished it.*

* *Beobachtungen über die sehr beträchtlichen Gebirge und Rotation der Venus*, with three copperplates. Erfurt, 1793.

In the German original of my paper, the translation of which is published in the Philosophical Transactions,* it stands thus: "On the contrary, from the circumstance that NO SUCH "EVIDENT FLATTENED SPHERICAL FORM *is perceived in this* "*planet* (namely, at its poles) *as in Jupiter and Saturn,*" &c. &c.

The author indisputably agrees with me in all the truths there asserted. He has himself observed the flattened shape of Saturn at the poles more exactly than I, and even determined the proportion of the shorter to the longer axis. But in the translation, for the words "*abgeplattete kugelgestalt des Jupiter* "*und Saturn,*" is put "*flat spherical forms,*" &c. which he understood as if I pretended to have observed spherical spots on Saturn. The author might have convinced himself of the contrary, by comparing the German original in the possession of the Royal Society.

2. He considers it as an equally wonderful relation, that I have SEEN in Venus, in the same manner as in the moon, mountains and shadows of mountains, which were four or five times higher than our Chimboraço, and that I thence pretended to have determined the rotation of this planet; on the contrary, he considers this last as hitherto undetermined, *because* HE *has never found a trace of mountains,* and all his observations, for 16 years past, have been absolutely insufficient to ascertain it, *though nothing of that kind could well have remained hid from him.*

Here it is not myself, but the truth, that I undertake to defend; and I am convinced that if my memoir above mentioned, on the Rotation of Venus, had been already known,

* Observations on the Atmospheres of Venus and the Moon; their respective Densities, perpendicular Heights, and the Twilight occasioned by them. Phil. Trans. 1792.

and the author had compared the almost innumerable and various observations contained in it, which all agree in their result, he would never have made such a declaration. *I have myself also never actually* SEEN MOUNTAINS *in Venus* AS IN THE MOON, but only *deduced* their existence and height from the observed appearances. It is even impossible to see them, according to what I have expressly asserted in my paper on the Twilight of Venus; because, on account of the thickness of her atmosphere, we can never perceive the shades of land on her surface. But if the appearances observed by me and others are true, the result deduced from them is mathematically evident.

That I have seen, *not unfrequently*, the boundary of illumination irregular, is *nothing new*, nor does it afford me any further merit than that of *confirming* with many others, *an old truth*, which DE LA HIRE, and still more ancient good astronomers, provided with the best and most powerful telescopes of their kind, had long ago discovered in perfectly similar phænomena. So early as the year 1700, DE LA HIRE observed greater inequalities in the termination of light in Venus, than in the moon;* and the Paris Academy thence concluded that planet to have higher mountains. The sole addition, as far as I know, which I have made to the older observations is, that in the crescent phase of Venus, sometimes one horn is only half as broad as the other; and that sometimes, though not often, about the period of the greatest elongation, one end of the enlightened part appears pointed, but the other rounded off: appearances which others, who had not been apprized of what they were to see, have frequently perceived as well, and

* See *Mémoires de l'Acad. des Scienc.* 1700, p. 378.

in the same manner as myself. It is here scarcely necessary to remind the reader, with respect to the ancient observations, that in all those where no extraordinary light is wanted, particularly powerful telescopes are by no means required. I should indeed be surprised that the celebrated author had not, in all the time since 1777, perceived any inequality in the boundary of light, or other appearance of that kind, tending to confirm the existence of very high mountains according to the old observations, were it not that his bold spirit of investigation has been chiefly employed in making much more extensive discoveries in the far distant regions of the heavens, where he has gathered unfading laurels. In fact, the observations which he has communicated from his journal are *much too few* to prove a negative against old and recent astronomers. Without encroaching upon truth in the least, I could certainly produce more good distinct observations during many months, from 1779, when I began to examine Venus carefully, to 1793, when my memoir on her rotation was finished, than are adduced for a period of 16 years in the abovementioned paper of my opponent: having, in the latter years, observed this planet not only daily, but, as far as the weather and her position admitted, almost hourly through the whole day and evening. This, I think, is shewn evidently enough by the memoir already mentioned, in which only the later observations appertaining to the subject are inserted: and without such steady perseverance, my trouble for so many years would have been fruitless, as was the case with other observers; *for, in almost innumerable observations, the same thing happened to me as to the author of the paper in question, namely, I perceived neither spots, nor any other remarkable appearance, except the unusually quick*

decrease of light toward the boundary of illumination, which itself was not sharply defined.

It is right that every acute observer should be on his guard against a precipitation which often occurs, and not contradict respectable astronomers who have preceded him, if he should not at once, in a few observations, find those appearances in an object which such credible men have perceived, or deduced from their observations. The mischief thence arising may be important, and lead to more general error in proportion to the celebrity of the contradicting observer, because there are always persons enow who will adopt it as a truth without further examination. And yet there are many examples of this in the most modern history of astronomy. Thus, for instance, the old worthy selenographer HEVELIUS found some of the mountains of the moon to be more than $\frac{2}{3}$ of a (German) geographical mile in perpendicular height; and this truth stood more than 100 years in all the elementary books. Later astronomers measured only a few of those mountains, and partly not with all the requisite circumspection; yet concluded, from too few and insufficient observations, that HEVELIUS had given them much too high.* This was already received as true in the elementary books; notwithstanding which the excellent HEVELIUS was absolutely in the right, as is proved by my numerous and incontrovertible measurements.†

When, in the years 1789 and 1790, the ring of Saturn appeared as a straight line of light, I perceived only a few pro-

* See RÖSLERS *Handbuch der practischen Astronomie,* 1 Th. p. 441.—Philos. Trans. Vol. LXX.

† *Selenographische Fragmente,* § 34 to 82.

jecting luminous points on it till after October, 1789; but in February, 1790, incomparably more of them, in the frequent observations I made. These, in part at least, I considered not as satellites, but as true and large inequalities of the surface of the ring; and thence drew, on the strongest grounds of probability, the same conclusions as MESSIER and other respectable observers had done 15 and 30 years before; one of those deductions, and which seemed highly probable, was, *that the southern surface of the ring must have many more and larger inequalities than the northern.* These remarks had already been made known to the world, in the publications of the Naturalist Friends at Berlin;* when I unexpectedly read in the Philosophical Transactions a conclusion which discouraged me very much, that the astronomers who considered these projecting luminous points as inequalities of the surface, were mistaken, those appearances being occasioned by the satellites of Saturn: this conclusion was drawn from some new and excellent observations inserted in the paper itself, but which were continued only to November, 1789. However, so much the greater was my pleasure to find this assertion recalled in the next volume of the Transactions, where Dr. HERSCHEL, from those very projections, has made the important discovery of the rotation of the ring, and determined its period. Now, if there are really in the ring of Saturn such enormous inequalities, I do not see why my conclusion, deduced from so many agreeing observations, namely, *that the mountains of Venus bear nearly the same proportion in height to her diameter, as those of the moon do to the diameter of the moon,* should be thought a wonderful relation, especially since all my

* *Schriften der Naturforschenden Freunde.*

observations hitherto, as for instance those on the visible luminous spots in the dark part of the moon, on the apparent changes of the moon's surface, &c. have been confirmed by others.

From these remarks, the answer will readily present itself,

3. How the author of that paper could look upon my observations on the rotation of Venus as unfounded, though there are so many of them which agree together, and *he had not read and compared them;* and could think the period of rotation as much undetermined as before. Whoever deigns to bestow some attention on my memoir on the rotation of Venus, will soon find,

(*a*) That certainly I did not go to work carelessly, *but first* arrived *gradually* at an *approximate* estimation *by almost innumerable observations made in very different ways.*

Although I perceived, as early as in the year 1786, some luminous spots of Venus, which seemed to me to shew a period of rotation of about 24 hours, as DOM. CASSINI had also thought; yet I suffered them to lie unpublished six years, because I was doubtful whether some delusion might not have intermixed itself; until at length a favourable opportunity accidentally led me to pursue the investigation of this subject *in an entirely different manner.*

(*b*) It will also be found that the author, among his observations, which taken altogether are *but few, cannot shew a single one in which he observed at the same time with me.* But every person conversant in these subjects will agree with me, that in order to prove the inaccuracy of my observations, or at least render them doubtful, it is essentially necessary, *that an impartial observer should have* DIRECTED HIS ATTENTION WITH

EQUAL CARE TO THE SAME CIRCUMSTANCES AT THE SAME TIME, *and not have seen them the same as I have given them.* In my memoir, to which I here refer, those observations only which belong to the point in view are compared together; *but in other observations, almost innumerable, which I made partly before I had paid any particular regard to the inequality of the horns, and partly in the intervals, I did not perceive, any more than the author, either spots or any thing appertaining to the matter in question; and consequently our corresponding observations perfectly agree together.* It is, however, and will remain a truth, that there is no such thing as a monopoly of discoveries; one man may luckily observe something to which the other did not direct his attention *in the same manner,* although he viewed it at the very same moment. Thus, for instance, since HEVELIUS's time many observers, provided with sufficiently powerful telescopes, have examined the moon, without perceiving the immense southern *cordilleras* of her edge, the perpendicular height of which, by indisputable observations, amounts to something more than a geographical mile, and which I have pointed out and delineated in my Selenotopographical Fragments, under the names of Leibnitz and Doerfel. And yet these high mountains are really there, and afforded a magnificent spectacle at the commencement of the solar eclipse on the 5th of September last year, though they were not then exhibited in their greatest projection. So likewise it is true, that several of the many important discoveries, on which the author has founded his eternal fame, might have been made as well by other observers, who were furnished with good achromatic telescopes, if they had directed their

attention in the same manner to the same objects, with equal acuteness and perseverance.

Having premised these remarks, I can now communicate exactly, and according to their connection, my new Observations on the Planet Venus; and that they may, in various points, be more easily and better compared with the observations of my opponent, I will at present follow the order of my journal.

New Observations, confirming the Rotation of Venus, her mountainous Inequalities, and the Twilight of her Atmosphere.

Feb. 18, 1793, 5^h 50′ *p. m.* As cloudy weather had continued uncommonly long, and as the experience of many years had already shewn that little or nothing remarkable is to be expected, when considerably more than half of Venus is illuminated, I could not till this time proceed on the observations, the planet now approaching her greatest eastern elongation. With 160 of the 7-feet SCHRADERIAN telescope, I had, with the full aperture, such an extraordinary soft and clear image as I scarcely ever found in this planet. According to fig. 1. (Tab. XII.) both ends of the boundary of light appeared equally rounded, without any perceptible difference. There was, however, again, in the middle of the enlightened part, a kind of darker nebulosity, not quite clearly to be distinguished, which seemed to consist of two very slight nebulous spots. The light decreased to extraordinary dimness toward the boundary of illumination.

Feb. 26, 5^h 15′ *p. m.* An extremely remarkable observation. With 160, 288, and 370 magnifying power of the 7-feet SCHR.

I found, the image being uncommonly fine and soft, that as usual there was no spot, but that the northern end of the boundary of light, *a*, fig. 2. was most certainly rounded off beyond all comparison more than the southern; the latter appearing to run on rather pointed, with an inequality upon it, on which a dim greyish shadow was perceived.

At $6^h\ 20'$. In order to secure myself against deception, I desired my attendant, who came in at that time, and has remarkably good sight, with some practice, to observe whether he saw any thing particular; and what? The answer he gave, at the first sight, was, that Venus had *an evidently irregular form*; that *on the right* (southern) *end of the illumination she was pointed, the point having some shade on it, but that on the left she was oval.*

At $6^h\ 40'$, the difference began to be less striking; and having intermitted the observation in order to recruit my eye, I found at $7^h\ 30'$ both horns equally rounded, though with this difference, that at the southern one a small indistinct glimmering point of light, barely perceptible, often shewed itself at *a*, fig. 3. not on the rounded part, but close to it: this was seen with 288, as well as 160. At $7^h\ 45'$ I found it still the same; and likewise afterwards with the 13-feet reflector, which also shewed me the point. Soon after, Venus became invisible.

There was no nebulosity to be perceived as on the 18th.

Feb. 27. I wished much to examine the changes which might happen in the course of all this afternoon, but high light clouds prevented me. It was very remarkable, *that at* $5^h\ 40'$ *on this succeeding day, I saw most distinctly the same appearance as the evening before*, with 109 and 160 magnifying powers, only with this slight difference, *that the shadow, which shewed itself*

again at the southern point, as at a, fig. 4, entered westward a little further into the point; and it sometimes appeared as if the shadow would penetrate all the way through, and entirely cut off the point; moreover the northern horn was not quite so much rounded as the evening before. With both magnifying powers I saw likewise again, on the illuminated part, a very faint oblong nebulosity *b*, distant only about $\frac{1}{3}$ of the semidiameter from the external edge. For greater certainty I applied a power of 288 and 370, with which I distinguished the abovementioned form of the illuminated part, extraordinarily fine and distinct; I could likewise see, with all the magnifying powers, the darker indentation of shadow *a*, but not the very slight nebulosity *b*. The indentation of shadow was in length at least $\frac{1}{10}$ of the semidiameter; and at $6^h\ 11'$ it began to pass quite through, so that the southern horn appeared rounded like the northern, and the fine point, being now separated, looked like a glimmering dot of light close to it. I saw this separate point of light repeatedly, with 209 times, among other magnifying powers, very plain and evident, the image being soft; in different observations I found it always the same, whatever was the power; and at $6^h\ 19'$ the southern end appeared fully as round as the northern. I thought it remarkable, that at $6^h\ 25'$, a power of 288 shewed it smaller than it appeared with a less power. At $7^h\ 12'$ the point of light had vanished, as I perceived with both the 7 and 13-feet SCHRADERIAN reflectors. Mr. TISCHBEIN, the instrument-maker, who came in toward the end of the observation, saw it in the same manner. Both horns at this time appeared quite equally rounded; but a new remarkable circumstance was now first discovered by Mr. TISCHBEIN. He observed with both reflectors, that at the

northern horn, though rounded like the southern, *a brighter pointed small inequality* projected out from the faint boundary of light, as is expressed at *a*, fig. 5. It was difficult to distinguish, but his eye, more accustomed to microscopic objects, saw it alike with both reflectors, and *in the same place*; I perceived it also, though it was not striking. The observation was continued by both of us to 8h 30′, when Venus being sunk too low, began to be indistinct. At this time indeed I could no longer distinguish that fine point; but in every part of the field of the instrument something brighter appeared in its *fixed* place.

Whoever is pleased to compare these two observations impartially, I doubt will not consider them as illusions. To me they rather appear, in more than one respect, convincing and important. In the first evening, the southern horn, as two observers agreed, changed its form very quickly, that is *in* 15 *minutes*, so much that the difference between it and the northern was not nearly so striking as before. In the second evening, the air being clearer, and the image excellent, this change was still quicker; for *in* 11 *minutes, during the observation itself, the end passed very* EVIDENTLY *to the form of a separate point of light.* Supposing both changes to be the same, and produced by the rotation, the alteration to a separate point of light must have happened on the first evening, *at most* 11 minutes later than 6h 40′, when I intermitted my observation; that is, about 6h 51′; because on the second evening it took place in 11 minutes. But on the second evening, when I noticed this striking alteration, I no longer knew the time marked the evening before, and I now noted down 6h 11′. *Consequently this change took place the second time* VERY NEARLY *in* 24 *hours less* 40 *minutes; and*

from these two careful observations alone we may conclude, very probably, the rotation to be nearly 23 *hours* 20 *minutes*; which agrees extremely well with the approximate period of 23 *hours* 21 *minutes,* which I have deduced from observations of two years, in my circumstantial memoir already quoted.

Feb. 28, *from* $10^h\ 50'$ *to* $11^h\ 30'$, *a. m.* With powers 95, 160, and 209 of the 7-feet SCHR. I found no spot, and both horns perfectly alike; the light decreasing toward the boundary of illumination extremely plain, and the terminating arch of both horns, but *particularly of the southern,* rather unequal and knotty.

At $3^h\ 10'$ to $26'$, with 160, 209, 370, and 632, a fine image; the decreasing light seemed at the boundary of illumination to mix itself with the colour of the heavens, becoming equally faint. *Both horns alike oval.*

At $4^h\ 36'$, the same.

$5^h\ 4'$, no difference.

$6'$, still the same.

$7'$, the southern horn began to acquire a pointed shape.

$9'$, it appeared already pointed; the northern blunt as before.

$11'$, the southern exhibited the same appearance as both evenings before; and I likewise perceived something darker making an impression into it.

$17'$, Venus behind clouds.

$19'$, through light clouds her southern horn was perceived to be pointed in comparison with the northern.

$37'$, the same in some clear intervals. The northern horn appeared always blunt.

That the decrease of light toward the boundary of illumination, whereby that part of the disc becomes extremely dim, is no deception, appeared now evidently; for whilst the planet faintly glimmered through the clouds, I could often see only $\frac{1}{3}$ of her illumined part, reckoning from the outer edge, and sometimes only half.

$5^h\ 55'$. Venus shining out for a short time between the clouds, the same appearance with full certainty; and I remarked also again a slight darker indentation at the southern horn; *but the scene was by no means so striking as both evenings before.*

$5^h\ 59'$, the appearance changed; and

$6^h\ 7'$, this was found to be confirmed; but I could not with certainty discover a separate point of light; sometimes, however, though but seldom, there seemed a glimpse of it at the southern horn. Immediately afterwards Venus was covered with clouds.

$6^h\ 30'$ to $6^h\ 45'$. Venus shining in a clear sky, her southern horn was again, as at $5^h\ 6'$, rounded exactly like the northern; and with powers 160, 209, and 370, and a distinct image, I found no trace of a separate point of light. Comparing this third observation with the two former ones, it agrees very well to the minute; for now the southern horn had nearly the same appearance of being like the northern, at $6^h\ 30'$, as it had the preceding evening at $7^h\ 12'$, and therefore 42 minutes earlier; but in general it was evident that the appearance remained no longer exactly the same as on the two evenings before; and this difference may be easily explained by the very probable supposition of a libration, and that it is not a single mountain which occasions the appearance, but a considerable ridge, with

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many high points: moreover the clearing up or thickening of the atmosphere of Venus, which according to my former observations is pretty dense, and the effects of refraction, may have a considerable influence on such phænomena. Whoever has frequently observed in the moon the very striking variety in the projections of the high ranges of mountains at her edge, namely, Leibnitz, Doerfel, or d'Alembert, will more readily comprehend such effects of a libration.

The 1st, 2d, and 3d of March, bad stormy weather.

The 4th, 6^h to 6^h 30′, p. m. with 160 of the 7-feet SCHR. the image being extremely fine, I found *both horns equally rounded, without any difference.*

At 7^h, the same. But at this time there appeared, in the enlightened part, a slight nebulous shade, which, as is expressed in fig. 6, extended to the boundary of light. At 6^h, in the bright twilight, I had not remarked it; and I suspected it to be a sort of dazzling, though the image appeared uncommonly soft and distinct. The bad weather which came on soon after did not allow me to apply other magnifying powers and telescopes.

March 5, at 4^h 25′ to 35′ *p. m.* with the same power, I found the northern horn still rounded, and the southern somewhat *pointed, but not strikingly so.*

At 4^h 40′, with a power of 200, the same; and moreover a weak shadow was again perceived on the planet. *So likewise with* 288 *very distinct, and then with* 370 *extremely certain*; *but on the whole it was not striking*; for the southern horn also appeared somewhat roundish, and probably another person less accustomed to such observations, would not have remarked it.

At $5^h\ 45'$, the atmosphere being less clear, it was doubtful; and at

$6^h\ 35'$, it was quite certain that both horns appeared equally rounded, without any difference. I found neither spot nor glimmering.

From the 6th to the 10th of March, the learned and worthy Dr. CHLADNI, inventor of the euphon, observed with me; and having ascertained, by careful comparison, the extreme goodness of my reflectors, can bear witness of it.

March 6th, cloudy.

March 7th, noon and afternoon cloudy.

At 6^h *in the evening* I found, with the 7-feet SCHR. and magnifying powers from 160 almost to 400, both horns constantly the same, without any difference. So they appeared to me also with the 13-feet reflector; and with both instruments to Dr. CHLADNI.

The 8th March, at noon, the image of Venus but seldom appeared fully distinct. In the intervening moments of greater distinctness, Dr. CHLADNI remarked, *that though* both horns were roundish, yet the northern *was rather more pointed than the southern*. Afterwards I found the same thing. In the afternoon cloudy.

From 6^h *to* 7^h *in the evening*, with 95 to 288 magnifying power, I found *both horns equally round*, and no spot or any thing remarkable, though Venus did not appear perfectly distinct.

March 9th, $6^h\ 15'$. *p. m.* Venus being near her greatest eastern elongation, both horns appeared pretty pointed, with a power of 250, and a fine soft image; they were also *both alike*, but with the slight difference, that close to the southern horn

a very minute particle projected, which seemed to be rather separated from the rest of the enlightened part.

At 8^h $2'$, *the air being clear, a projecting inequality shewed itself with certainty at the southern horn, as is represented in fig.* 7, (Tab. XIII.) *at b.* It was found the same with 288 of the 13-feet.

As our own atmosphere was then very clear, that of Venus also seemed to be purer than usual; for with both reflectors, and particularly with the 13-feet, *Dr.* CHLADNI, *as well as myself*, enjoyed a magnificent view of the arch of illumination, which seldom presents itself so well to the eye, *the image being uncommonly clear and distinct. To both of us the boundary of illumination, toward which the light became very dim, appeared* (be it ever so much contradicted) *not only nebulous, and not sharply terminated, though sensibly sharper than usual, but also very evidently unequal and rugged, with faint shades between*, as I have often seen it, but never so plainly. In truth, the appearance, as each declared, was *very like* the image of the moon at the time of her quadratures, only that the boundary of light was sensibly less sharp, and the faint shadows between were not almost black, but in some measure like the dark spots of the moon's surface, grey, yet darker than the other parts. This instructive observation remains still before my eyes. So delicate a picture of nature cannot well be drawn, however we both made cursory delineations of it, from which fig. 7. is copied: but at the boundary of light, soft grey shadows must be imagined, traced into the interstices at *a, b, c, d, e, f, g*.

March 11*th, from* 6^h $10'$ *to* $45'$, *p. m.* the weather having cleared up after snow, I found *no striking difference of the horns*, with powers of 209, 288, and 370, and a distinct image; however, the southern appeared rather less pointed, which was

occasioned *by a* VERY FINE *glimmering pointed line of light, that ran on from the horn not far into the dark side, as at a, fig.* 8. *and was visible with all magnifying powers. I saw this line of light equally, whether I observed with the whole aperture, or covered a considerable part of it.*

It would be singular indeed, and most discouraging for all such observations, if so many appearances, agreeing together, and viewed with every precaution, should be merely deception, particularly as they usually and principally occurred only at the southern horn, without any reason that could be assigned if it be thought a fallacy. But if there be no deception, it follows incontrovertibly, that the surface of the southern hemisphere of Venus, like that of the moon, has the most and greatest inequalities.

March 12th, 6^h $15'$ to $30'$ *p. m.* no kind of difference in the horns, no spot, or any other unusual appearance, could be seen with a power of 209.

At 8^h, the same.

But on the 13*th of March, from* 11^h *to* 11^h $20'$ *a. m.* I perceived, with the *same* magnifying power, *a very evident and remarkable difference. The northern horn appeared pointed, but the southern was rounded, with a very small knot close upon it to the south,* as at *a*, fig. 9. Thus I saw it with 160 and 288 magnifying powers; and I even distinguished it with 95, though this was too small a power for so minute an object. On the northern horn I found nothing similar, notwithstanding I compared them repeatedly. Business called me away; and the atmosphere soon afterwards became cloudy, and continued so all day.

This very remarkable observation is indeed not precisely the

same as those of the 26th and 27th of February: yet the appearance is very little different from that of the abovementioned days, when the shadow, fig. 4. at length penetrated quite through, and the separated part was perceived as an insulated bright point. Now if it be considered, that on the 28th of February, only 24 hours later, this appearance recurred, but was not exactly the same; and that when a very extensive mountainous southern region forms the edge of the planet in various degrees of obliquity, according to the respective situations of Venus and the earth, the phænomena must naturally be so diversified; there cannot be the least doubt, but that the same southern range of mountains, which occasioned the similar appearances of the 26th, 27th, and 28th of February in the evening, also produced this of the forenoon about 11 o'clock, according to the rotation; especially as no intervening observation contradicts this conclusion. The effect of small differences in the position of planets, may be exemplified from the late eclipse of the sun on the 5th Sept. 1793, when the projections of the mountains Leibnitz and Doerfel, bounding the southern edge, were so different from those of the older observations, under a similar variety of circumstances. The abovementioned conclusion with respect to Venus becomes still more evident and remarkable, from its *agreeing more exactly than could be expected, according to the circumstances, with the period of 23 hours 21 minutes, which, in my memoir on the rotation of Venus, I had determined as near the truth:* for on the 27th of February that appearance took place about 40 minutes earlier than the evening before; and the middle of the time when the southernmost part of the southern horn appeared as a separated point of light (a phænomenon similar to the present),

was by that observation at $6^h\ 29'$. *From the 27th February, 1793, $6^h\ 29'$ p.m. to the 13th March 11^h a.m. there are 13 days 16 hours 31 minutes, which, with the period of 23 hours and 21 minutes, are resolved into 14,04 revolutions, exact to the very inconsiderable fraction of* $\frac{4}{100}$; which is so much the more surprising, as no attention could be paid to the inequalities.

The same day at 6 p.m. I saw Venus with a power of 160, very sharp and distinct through thin clouds; and found both horns again equally pointed, and the much fainter light at the boundary of illumination very evident. And the weather *on the 14th of March,* having been bad all day, I saw, together with my attendant, the same thing on the

15th of March, at $6^h\ 30'$. Both horns were then alike, and there was no spot.

March 16th, $2^h\ 15'$ to $45'$, both horns equally pointed; no spot. To search with the greater certainty whether I could not discover some inequality, I took the 13-feet reflector, and still found it as before, the image being uncommonly sharp. Thus one observation gives weight to the other against fallacy.

From the 17th to the 21st of March, variable and cloudy weather.

March 21, at 7 in the evening, with powers 160, 288, and even 95, of the 7-feet, both horns were pointed, without any perceptible difference: no spot.

March 22d, $2^h\ 35'$ p.m. the same.

At 7^h in the evening, however, I found *a sensible alteration,* with 160, 209, 288, and 370 magnifying powers. *The northern horn constantly appeared, according to fig. 10, not pointed as before, but somewhat less obtusely rounded,* whilst *the southern* was pointed and *projecting a little* beyond the line *of the cusps.*

Between the projecting point and the enlightened side, there was often to be perceived, and equally with all magnifying powers, a light greyish shade, which seemed to divide the point. Soon after the weather became cloudy.

March 23d, 6^h 37′, the atmosphere having cleared up much, but the air being still not very favourable, I found, with the same magnifying powers, *an exactly similar appearance*; *but an hour afterwards, the northern horn ran out in the same manner into a point, and projected as far as the southern, so that the phænomena were no longer the same.* Soon afterwards it became cloudy.

March 26th, 6^h 10′ *p. m.* the weather having cleared up again, I saw *both horns equally pointed*, with the same magnifying powers.

7^h 30′, *the same.*

8^h 15′, *also the same.* This too agrees with the period of rotation, according to which the phænomena, observed on the 22d and 23d at 7^h and 6^h 37′, could not be visible again at the times here noted down.

March 27th, 11^h *to* 11^h 40′ *a. m. both horns equally pointed*, and, as usual, no spots. With a reference to my former remarks, I had proposed to observe Venus every hour throughout the day; but it grew cloudy.

At 6^h 30′ *p. m.* the sky having cleared in the part where Venus was, I found in like manner *both horns* equally pointed.

At 7^h 30′ *p. m.* the same.

March 28th, 10^h *forenoon*, with 160, 209, and 288, *both horns were pointed*, without any striking difference.

11^h 15′, with the same, both horns *equally pointed*.

5^h 30′, *the same*; even with a magnifying power of 370 times.

6^h 10′, *just the same.*

March 30th, 6^h 45′ *p. m.* with 160, both horns uncommonly sharp, and *equally pointed.*

7^h 30′, the same. No spot. Then followed rain and cloudy weather.

April 2d, 6^h 50′ *p. m.* with power 160 of the 7-feet SCHR. it struck me *with uncommon certainty and precision, after so many similar appearances of both horns, that the southern horn b, fig.* 11, *was remarkably slenderer in comparison with the northern, a; and that in general the whole southern illuminated part, c, b, d, appeared considerably smaller than the northern, c, a, d.* I tried this phase with 288 and 370, and found it to be *assuredly so*; *and with the same certainty I observed it also repeatedly confirmed with the noble 13-feet reflector*, till 8 o'clock. *My attendant, who knew nothing of it, made the same remark, and particularly noticed the irregular form of the arch bounding the illumination*, which, by entering in further from *d* to *e*, than from *d* to *f*, formed a slenderer horn, as often happens with the moon; and also in the same manner in its single parts, the crescent of Venus *appeared uneven, like that of the moon*, although not sharply so, but faintly and undefined. I did not now see the mountains of Venus, by their projection and shadow, as in the moon; but the appearances above described must indisputably have been occasioned by mountainous inequalities. Very often have I perceived similar phases on the moon with my naked eye.

It would be inexplicable, if different eyes, with different excellent telescopes, and various magnifying powers, should have seen for an hour together such an appearance, with equal confidence, and yet the whole be nothing but a fallacy, misleading

a careless observer. Did not CASSINI, BIANCHINI, and other observers, surely not deficient in caution, perceive similar phænomena, and draw the same conclusion?

At $8^h\ 35'$, Venus presented not a clear image. She had already passed the pleiades about half a degree, and my hope of seeing perhaps an occultation was frustrated.

$10^h\ 15'$. A very instructive observation, by comparison with the preceding. Notwithstanding Venus was got near the horizon, and had some tremulous motion from the fine vapours, the sky being otherwise clear, yet her image was free from false light, and sufficiently distinct, with power 160 of the 7-feet SCHR. a reflector which almost never fails me. *I was quite surprised to perceive most evidently, at the first sight, that the abovementioned remarkable phase had changed as remarkably within 2 hours 15 minutes; and that, even whilst the instrument was screwing to its focus, in all parts of the field, the northern horn a, fig. 12, constantly appeared pointed; whereas the more slender point of the southern horn, b, had vanished, and this horn had become rounded, as it was on the 26th, 27th, and 28th of February, and the 13th of March.*

Comparing this observation with those I have here named, it becomes very remarkable and decisive, by confirming my former approximated estimate of the period of rotation. On the days just mentioned I had, at the hours noted down, observed a somewhat similar change in the southern horn, *conformably to such a period of rotation;* but had never seen it again in all the numerous observations I made since the 13th of March, at hours when, according to the rotation, it should not appear. But now it was seen again at $10^h\ 15'$ in the evening. *From 11^h in the forenoon of the 13th of March, to the*

2d of April at 10^h $15'$ *in the evening, there are 20 days* 11 *hours and* 15 *minutes, which, with a period of rotation of* 23^h $21'$, *divide into* 21,005 *revolutions, exact to the inconsiderable fraction of* $\frac{5}{1000}$.

April 3d, 5^h $40'$ *p. m.* with 160 and 370 magnifying powers, I found Venus again *irregular* in single parts of the *arch terminating the illumination.* That is, according to fig. 13, (Tab. XIV.) it sunk in somewhat, but very little, at *a*, and between *a* and *b* it protruded out a very little. Both horns, however, were pointed, and no spot could be seen.

At 6^h $48'$, the boundary of light went in a little at *d* also, according to fig. 14.

At 7^h $25'$, I found both horns alike pointed, and no striking difference whatever, as the evening before. No spot.

At 8^h $10'$, the same. No perceptible difference in the horns.

At 9^h $50'$, I found *the southern horn visibly, though not much, rounded as yesterday. Mr.* TISCHBEIN *saw it so likewise:* but Venus was already too low, and undulated in the vapours, so that we could not reckon on this observation with confidence; yet it agreed with the former.

April 4, at 5^h $50'$ *p. m.* with a magnifying power of 160 Venus appeared extraordinarily plain and fine, but without spots. The light lost itself in a dim grey at the boundary of illumination, which appeared somewhat uneven, as it did yesterday about the same time, but both horns looked equally sharp.

Without thinking of it in the least, I saw, with a power of 288, *that the southern horn was somewhat slenderer than the day before yesterday;* and this was confirmed with a power of 370,

which shewed me clearly that the smaller form of the right side, according to fig. 15, was occasioned by the boundary of light running in a little more at the right horn.

6^h 25', I found this repeatedly confirmed with 370.

At 7^h 5' *to* 10', this difference no longer struck my eye; both horns appeared equally pointed.

8^h 24', the same with 160. Venus was no longer distinct.

April 5th, 5^h 15' *p.m.* Both horns indeed sharp, but *all* as it was the evening before, and nothing striking, with 160.

5^h 25', the same with 288.

6^h 38', still the same.

7^h 38' *to* 8^h 10', with both magnifying powers, and afterwards with 136 of the 13-feet, no manner of inequality in the horns. With the greater telescope the decrease of the light to dimness, and the *dim unevenness of the boundary of light*, appeared extraordinarily fine.

8^h 42'. Both horns still equally pointed, with power 160 of the 7-feet. No spot.

9^h 55', still the same. The planet being now as low as on the 2d and 3d of April about the same time, I tried, by screwing in various ways, whether I could get the southern horn to look somewhat rounded, as it did then, but in vain: both horns were equally pointed.

April 6th, 6^h 45' *p. m.* with 160 of the 7-feet, I found no striking difference, *both horns being equally pointed.* No spot.

7^h 29', likewise so.

8^h 10', the same with power 288, and an extremely sharp image.

8^h 45', the same.

$10^h\ 5'$. In this situation of Venus near the horizon, I tried again, by screwing the small speculum, and moving the image in the field, whether I could give her a false form, similar to that of the roundness of the southern horn on the 2d and 3d of April; but both horns were, and remained, pointed. Consequently the observations of the 2d and 3d April were no deception, and they agree extremely well with the period of rotation, being the 5th and 6th new repeated proofs of it.

April 7th, $6^h\ 30'$. With power 160, both horns were equally pointed: no spot, nor any sensible inequality, except the dim faintness of the boundary of light.

$6^h\ 55'$, with 288, the same.

$7^h\ 15'$, the same.

$7^h\ 55'$, still the same.

From the 7th to 12th of April cloudy weather.

April 12th, $6^h\ 30'$ *p. m.* With the same magnifying power *both horns equally pointed. However, Venus was now become too narrow a crescent for a rounded shape of either horn to be expected.*

$8^h\ 20'$, the same, without perceptible inequality.

A series of changeable bad weather.

But on *the 22d of April in the evening*, the hour not being marked in the journal, *the southern horn* appeared to be illuminated *only half as broad as the northern.*

April 23d, $5^h\ 45'$, *till after* 6^h *p. m.* With 160 and 288, Venus was *distinct, and her southern horn again much smaller than the northern, according to fig.* 16.

But at 10^h *there appeared no longer any striking difference.* However Venus was already got too low, and I would never advise a careful observer to choose such a time for investigations of this kind.

April 30th, 7^h. Judging from the outer circle, I found the northern horn running out much longer than the southern. At the same time the southern appeared sensibly smaller: see fig. 17. I leave these remarkable phases to the judgment of the skilful, but to me they seem inexplicable, except from real shadows of an uneven mountainous surface.

May 3d, 7^h p. m. After much rainy weather I saw a similar phase; for though I found both horns, at 7^h 30′, without any sensible difference in their length, yet the northern was evidently broader than the southern.

7^h 45′, still the same.

8^h 25, the southern horn was still somewhat smaller, but only a little.

9^h 45′. Venus being now near the horizon, and undulating in the vapours, I could perceive *no difference in the breadth of the horns.*

May 6th, 5^h 50′ p. m. with a very distinct image I found both horns perfectly alike.

May 8th, 8^h 15′ p. m. the same, but the image indistinct after storms. No spots; but they are not to be expected in these small phases.

I now longed for fair weather, that I might carefully attend to the twilight from the atmosphere of Venus, which I discovered in 1790, as far as should be practicable in the present less favourable circumstances.

May 9th, 6^h 25′ p. m. I found, with full certainty, that though both horns were equally long, the southern at *a*, fig. 18, was *scarcely half so broad as the northern* at *b;* and this was confirmed by continued attention to the object.

7^h 50′, still nearly the same.

At 8^h 20′, on the contrary, the difference was no longer by far so perceptible.*

This day the first traces of Venus's twilight shewed themselves; for the points of the horns appeared to terminate beyond the illuminated hemisphere, in an extremely faint bluish-grey light.

May 10*th*, 6^h 40′. A perfectly similar phase. I found, so as to be quite certain of it, the southern horn only half as broad as the northern; but both horns were equally long.

7^h 30′, still the same.

8^h 15′. With 180, 400, and 560 magnifying powers of the 13-feet reflector, and a distinct image, I found traces of the twilight which could not be mistaken. The light grew dimmer and dimmer to the point of both horns, and at the points was so dim, that it seemed to lose itself in the faint light of the sky. A still finer dimmer trace of light shewed itself twinkling at both sides, on the edge of the dark hemisphere, and including this the two horns comprehended sensibly more than a semicircle; but it was too fine and dim for me to measure its extension.

Even if I had not seen this, I should repeatedly have obtained conviction of the particular density of Venus's atmosphere, by the faint colour of the points of the horns, and of the boundary of illumination.

* It is scarcely necessary to put the reader in mind, that small, undulating, knotty inequalities of the boundary of light, in such observations, must not be taken for true inequalities, or mountains of Venus. In general, these small crescents, as the enlightened part lies obliquely to the eye, are not well suited for observing the true inequalities of the boundary line, or any spots there may happen to be. For such observations, we should be assiduous in attending to the planet, about the time of its greatest distance from the sun.

In this reflector likewise, as well as in that of 7-feet, the southern horn appeared sensibly smaller than the northern.

May 12th and 13th, I perceived again traces of the twilight of Venus; but the stormy state of the air rendered it too bad for such nice observations.

May 16th, after sunset to $8^h\ 40'$, I had, for the third time, the pleasure of observing this crepuscular light of Venus's atmosphere, with the 13-feet reflector. Although the circumstances were not by far so favourable for such observations as when I discovered it in the year 1790, and the luminous appearance therefore came to the eye sensibly weaker and more indistinct than at that time, yet all was confirmed; and in this observation I thought it worth remarking, that the dim crepuscular light seemed to extend sensibly further on the southern than on the northern horn, though this might easily be a deception.

May 19th, after sunset, the light now coming to the eye sensibly clearer, I found the circumstance just noticed to be again the same, with 97 of the 7-feet HERSCH. and 136 of the 13-feet.

Hitherto the circumstances had not been favourable enough for a repetition of the measurement, and therefore I was eager for a better observation.

But *May 20th*, Venus was covered with clouds. However, at length I succeeded in a measurement,

May 21st, at $8^h\ 30'$, *p. m.* six days before the inferior conjunction, and consequently just the same time as in the year 1790. Venus being rather too low for the 13-feet, and for the 7-feet HERSCH. I employed the 7-feet SCHR.; and found the crepuscular light beautiful, and sufficiently distinct. *It*

extended, according to fig. 19, (Tab. XV.) *from the proper points of the horns a, b, a considerable way, on the edge of the dark hemisphere, to d, e; and equally far on both sides, having the appearance of a very dim, constantly decreasing light.* But I must remark, that in the present more unfavourable situation of Venus, it did not affect the eye as a bluish-grey light, which was its appearance March 12, 1790, but only as a dim grey light.

According to my usual projection-measure,* in which each decimal line of the projection table is equal to 4″ of space, I found the apparent diameter of the planet *a c b*, after repeated trials, = 15 lines = 60″; the projection of the crepuscular light running into the dark hemisphere *a d*, *b e* = 25 lines = 10″, *and fully so, being rather more than less.*

As the crepuscular light could be distinguished from that of the points of the horns, by its sensibly fainter colour, I was able to measure it from the points. But in order to know with certainty whether I had taken the true termination of the

* In the year 1790, as well as in 1793, I measured this crepuscular light with a projection-machine, which is nothing more than a very simple projection-micrometer, useful in many cases, both by day and night: it gives, for all magnifying powers, the measure of the projected object immediately in minutes and seconds of space, without the necessity of first measuring a fundamental line. I contrived it for my purpose of a selenotopography, and constructed it myself. After an experience of many years, I certainly would not lay it aside, in most cases, it being so quick in the use. I have described it, in all its simplicity, in my "*Beyträge zu den neuesten astronomischen Entdeckungen,*" p. 210, where the *older* lamp-micrometer of the worthy Dr. HERSCHEL is *also described* BEFORE, *p.* 138, with which this machine may be compared. It has never made pretensions to be a new invention, because projection-micrometers of many kinds, for example, accompanying microscopes, have long been known. I remember with pleasure that, even in the year 1778, the window frames were my projection-micrometer, on which I determined the proportion of magnifying powers to one another.

horns *a*, *b*, for the foundation of my measurement, I measured likewise the two lines *a f*, *b g*, perpendicular on the line of the cusps. I found the northern side $f\,a = 8$ lines $= 32''$, the southern only FULLY 7 to $7\frac{1}{2}$ lines; mean, $7{,}25 = 29''$; consequently both sides together $f\,a + b\,g = 61''$, therefore the mean of each side $= 30'',5$; but if the southern side *b g* be put $= 7{,}5$ lines, the mean will be fully $= 31''$; so that, as the semidiameter, according to the first measurement, could amount only to $30''$, I probably observed the cusps as projecting $0'',5$, and perhaps something more, beyond their proper line;* and consequently *the projection of the crepuscular light, which extended into the dark hemisphere, was certainly and at least as* 1 : 6 *in proportion to the apparent diameter.*

My success in this measurement was the more lucky, as on the 22*d of May* Venus could no longer be discerned, though the air was clear.

These are my late observations, made about the time of the greatest eastern elongation, in the year 1793; and continued three months to the inferior conjunction. Under my present circumstances, I hope to be excused for giving them with such prolixity; but I should quite weary the reader, were I now to lay before him likewise my further observations, continued to the last western elongation; which, therefore, I shall rather reserve to another occasion, especially as they contain little that is interesting.

However, I must not leave unnoticed some conclusions, remarks, and explanations, which are deducible from these observations; and which have for their object, partly the moun-

* The remarks and computations that follow hereafter, will shew that the penumbra was probably included in the measurement.

tainous inequalities and period of rotation which I formerly discovered, and partly the atmosphere and crepuscule of Venus.

I. *Remarks on the Mountains and Rotation of Venus.*

1. As I have already said, I gave, in the memoir on this subject published last summer, only those observations which particularly belonged to the object, out of a *very great number* that I had made during 13 years; and I omitted the rest, because otherwise they would have amounted to a volume alone. Now with regard to those I have communicated, and which shew the real existence of considerable mountains, as well as an approximate determination of the rotation, the respectable author of the paper against me has not observed the planet Venus *once at the same time,* which might easily be the case in only 38 observations, that are adduced from a period of 15 years. But in the numerous remaining observations, I saw neither mountains, inequalities, nor spots, any more than the author; and I doubt not, that among these observations I should find many which were made at the same times as when he observed. The same holds good

2. With respect to the new observations for three months, here communicated, which amount to more than 100, and were made at various hours, and on different days. Of the 25 adduced by my opponent, there are *only* 4 made nearly at the same hour, which is the chief circumstance; and not only *in all these, but likewise in very many other observations, I saw, exactly as he did, no spot, and both horns like each other:* so that of all his observations, not one contradicts mine. And yet it would

not be a decisive contradiction, if some observations made at the same time by another person, were in opposition (though that is not the case) *to so many of mine, made in various ways, yet agreeing together;* because, when fallacy of vision is in question, it may always be doubted which of the two observers is deceived; since this depends on the goodness of sight and of instruments, but much more on care and caution.*

I confess impartially, that, before reading the observations contained in my two memoirs, I should have formed the same judgment from those of the abovementioned author that he has done; and on that account his paper is highly valuable to me, as leading to a more scrupulous examination of new truths.

3. However, that which these new observations, here communicated, clear up and confirm, in correspondence with my older ones, on the mountains and rotation, is, that the planet Venus has very considerable mountains and elevated ridges; and indeed the most and the highest in her southern hemisphere. This appears

(*a*) From the observations of the boundary of illumination, which is not sharply terminated, and seems formed of light and greyish shadow indistinctly intermingled. This is chiefly to be perceived only about the time of the greatest elongation,

* By comparing the respective times of the two observers, it appears that both of them viewed the planet on the 4th of April at 7^h 30′, *p. m.*; the 5th April at 6^h 38′, the 6th April at 6^h 38′, and the 7th at 7^h 15′, *exactly at the same time, and saw exactly the same appearance.* The comparison of these observations is the more instructive, because I did not, like my opponent, observe Venus only once, but as often as was possible each day, and *at other times, on the same days, found evident changes*; for this shews plainly enough, that whoever wishes to see the same, and as much, in Venus, must observe with equal industry, and on each day as many hours as possible, with the same care.

when the eye looks perpendicularly through the dense atmosphere of Venus, and by no means in the small crescent form of light, when the lines of vision are much longer and more oblique through that atmosphere: it is in the former position of the planet alone that it can be seen distinctly, but even then not always equally so. One of the finest scenes of this kind was afforded (for example) by the observation I have adduced of the 9th, when Dr. CHLADNI viewed the planet with me. A less striking inequality, though perfectly certain, was discovered by my learned friend Dr. OLBERS, July 31, 1793, at $11^h\ 5'$ in the forenoon, which we both observed and delineated in the same place, and exactly similar, after we had been observing since $3^h\ 15'$ in the morning, but till that time saw no inequality. Were these small indentations or darker places merely atmospherical, no reason can be perceived why they should shew themselves only in the boundary of illumination, and not in the other enlightened parts also.

(*b*) The same thing appears, moreover, from the irregular form which the arch bounding the illumination sometimes assumes, and from the phænomenon thence arising of the much smaller size of one horn, and particularly the southern, in the crescent-shaped phases of the planet; as is shewn, on the same grounds, by the observations contained in my former memoir on the rotation.

Were these observations, as is alleged of the rest, nothing but fallacy, I should wish to know the reason, why that deception happens only sometimes, continues only some hours, and almost always takes place on the southern horn only, very seldom on the northern. Whoever compares together the obser-

vations of this kind contained in my memoir on the rotation, to which I have referred, § 12 to 23, will find 14 in which the southern horn appeared much smaller than the northern, but only one or two instances of the opposite phænomenon. And, if it were merely deception, why does the smaller horn, when the planet is seen through light clouds, always disappear sooner than the broader one, and become visible again later? (See § 12. No. 4. of the memoir.)

It further appears likewise,

(*c*) From the observation, that *sometimes, though much seldomer*, one horn, and particularly the southern, is seen rounded *about the time of the elongations*, but the other pointed. And by this very circumstance chiefly is

4. The period of rotation, which I had concluded to be nearly $23^h\ 21'$, confirmed and rendered evident by the new observations given above.

Having already explained this curious circumstance when the observations themselves were stated, I will here only make the following remarks.

(*a*) If the very remarkable observations of the 26th, 27th, 28th February, 13th March, 2d and 3d April, when the southern horn appeared rounded, but the northern pointed, are compared together, the abovementioned period will be found to suit them all, during an interval of 37 days, as exactly as can possibly be expected, and indeed to very inconsiderable fractions. If, on the other hand, they are compared with the older observations of this phænomenon, namely, those of 28th December, 1789, 31st January, 1790,* the 25th, 27th, and 30th

* See *Selen. Fragm.* § 522.

Dec. 1791, and the 11th Jan. 1792,* the differences are more considerable. Thus, for example, from the most distant observation, on the 28th Dec. 1789, at 5^h *p. m.* to the 27th Feb. 1793, at 6^h 41′ *p. m.* are 1157 days 1 hour and 41 minutes, which dividing into 1189,28 revolutions, might occasion some doubt. But,

(α) In each separate period, several observations correspond as well as can be desired:

(β) The period is only assigned nearly, but the interval of more than four years is very long, so that an error of seconds may occasion such an excess; and accordingly the abovementioned time would divide even with a period of 23^h 21′ 19″: and

(γ) In such computations, no regard is paid to the inequalities of the planet, nor to the middle of the duration of the phænomenon: wherefore so considerable a length of time can never be divided exactly by the period; as my observations of the rotation of Jupiter likewise could not, under similar circumstances, though the period of that rotation is sufficiently well known.†

(*b*) A like doubt might arise from the phænomenon being sometimes not at all or very doubtfully perceived, about the times of the greatest elongations, even at the hours when it was to be expected, according to the period. Hitherto, however, during more than four years, only three instances of this have occurred to me; which were in the years 1790 and 1791, and about the time of the late western elongation, in August, 1793; in which last I only *twice* perceived *barely a trace* of a somewhat rounded form on the southern horn. Moreover, as often

* See *Beob. über die sehr beträchtlichen Gebirge und Rotation der Venus*, § 26 to 30.

† *Beyträge zu den neuesten Astronom. Entd.* p. 1 to 138.

happens, the weather was not always favourable; and besides, the observations already communicated contain sufficiently evident marks of a libration, whence such cases may be easily explained. So, for example, the mountainous ridges of the moon's southern edge, *Leibnitz* and *Doerfel*, do not shew themselves quite clearly at each rotation, but only sometimes arrive at their full projection.

(*c*) But the very circumstance, that during more than four years, in so great a number of observations, I have perceived this phænomenon only ELEVEN TIMES with perfect certainty, and only a few other times uncertainly, and that in all the intervals I have expected it in vain, notwithstanding my frequent wishes, seems alone to shew, evidently enough, that I cannot have been deceived; especially as those appearances have been seen, with various magnifying powers of different telescopes, and in several instances with different eyes, perfectly alike, and with full certainty; and it is not reconcileable to our understanding, how such a fallacy should, at different times, always preserve one and the same period.

The following example, which I here take an opportunity of adducing as remarkable, may shew how cautious we ought to be, in drawing conclusions from our own observations, against the truth of those made by others. Jan. 5, I reviewed with the 13 and 25-feet reflectors the Mare Crisium (HEVEL. Palus Mæotis) in the moon, and made some observations. The following day, Dr. OLBERS of Bremen, who now pursues his observations with an extremely good 5-feet DOLLOND of $3\frac{3}{4}$ inches aperture, mentioned to me, that he had discovered *the preceding evening*, in the *Mare Crisium*, between Picard and Auzout, two small craters in the grey plain,

which were both wanting in my topographical charts; and about which, therefore, the question might arise, whether they were not newly produced?—I had seen nothing of them with my more powerful instruments. Again, on the 6th, I examined the part of the surface which he had exactly pointed out, with powers 136 and 300 of the 13-feet reflector, and found nothing. The 17th, I looked for them with the 7-feet, in vain. I did the same on the 3d February, with 179 of the 25-feet, and likewise on the 6th, but found not these craters. Hence I might have concluded, with probability, that the learned observer had been exposed to some deception; and perhaps I should have been believed. *And yet Dr.* OLBERS *was perfectly in the right.* On the 6th of March, I readily found the largest of these two craters, without seeking for it long, and saw it *uncommonly sharp and clear*, with 160 and 280 of the 7-feet SCHR. It is very nearly as big as a crater which I discovered last year, lying also in the plain, between the eastern bounding mountains, where they break down; it is surrounded with a broad, and proportionably flatter, annular elevation, of little brightness; it appears to be uncommonly deep, in proportion to its breadth; and if a straight line be conceived, running from Picard* towards the middle of the southern boundary mountain, which projects inward in the shape of a wedge, it lies on this line about $\frac{2}{3}$ distant from Picard. As I have examined this tract of the Mare Crisium very often, and under the most favourable angles of illumination,† in searching for the veins of mountains, or the flat mountainous layers to be found there, but *never perceived the slightest trace* of these craters, the

* See Tab. VI. of the *Selenotop. Fragmente.*

† See Tab. XXXIII. XXXIV. and XXXV. of the same work; and § 355 to 397.

observation of Dr. OLBERS is certainly not unimportant, and it will on occasion be further explained.

If any astronomer shall think it worth the trouble to observe *Venus, not barely now and then, at whatever time of the day it may be, but continually, with the same persevering zeal, and when the weather is favourable almost hourly, about the time of her greatest distance from the sun,* I am convinced that he will certainly perceive the rare phænomenon in question, just as well as I have done. If, contrary to all reasons which hitherto appear, I should hereafter be convinced that I was deceived, I would myself, willingly and impartially, bring the offering to truth; and so much the more readily, as no indirect views have ever led me on, but I have been actuated solely by an irresistible impulse to observe; and because I certainly shall never have reason to be ashamed of the observations I have laid before the world, which have always conducted me to new truths.

II. *Further Explanation and Correspondence of Computations of the Twilight, together with Remarks on the other Properties of the Atmosphere of Venus.**

As the celebrated author of the paper so often mentioned, "on the planet Venus," though he confirmed my discovery of the twilight of Venus's atmosphere, *yet represents the computation of it, p.* 16 *and* 17,† *as not demonstrated, and positively as very inaccurate,* which may, without any foundation, be injurious to the truth, it becomes my duty to give some explana-

* Many of the explanations and remarks in this section come from Dr. OLBERS of Bremen, who, at my request, kindly undertook not only to examine the old computation, but also to compare the calculations deducible from the new observations.

† P. 214 and 215, Phil. Trans. for 1793.

tions and remarks, that persons skilled in those matters may be better able to form a right judgment of my new computation, which agrees excellently with the old one; and at the same time may determine, whether there be inaccuracy and error, and on whose side it lies.

1. The first objection is concerning the apparent diameter of the sun, as seen from Venus, which I have assumed at 44′, in the computation of the penumbra, smaller, it is alleged, than I ought to have taken it.

M. DE LA LANDE puts the diameter of the sun in the apogee $= 31' \, 31'' = 1891''$. Now the apparent diameter seen from Venus

$$= \frac{1891'' \times \textit{dist. } \odot\textit{lis in apog.}}{\textit{dist. Ven. a sole}};$$

consequently,

$$\begin{array}{ll} \text{log. } 1891 & = 3{,}276692 \\ \text{log. dist. } \odot & = 0{,}007231 \\ \hline & 3{,}283923 \end{array}$$

log. of the distance of Venus *in aphel.* -	9,862318
log. of the distance of Venus *in perihel.* -	9,856337
log. of the diameter of the sun *in aphel.*	3,421605
log. of the diameter of the sun *in perihel.* -	3,427586

Diameter *in aphel.* $= 2640'',0 = 44',0$

Diameter *in perihel.* $= 2676'',6 = 44' \, 36'',6$

But if the assumed diameter of the sun in the apogee 1891″ be corrected for the irradiation, which may be put $= 6''$ (DE LA LANDE *Astron.* § 1388), we have

the diameter of the sun seen from Venus

in aphel. = 43′ 51″,6

in perihel. = 44′ 28″,1

I really do not see, therefore, how the diameter of the sun seen from Venus could be expressed generally, and with respect to every part of her orbit, more accurately, than as 44′, the quantity taken for the calculation. And indeed equally unimportant, must be considered

2. The remark on my computation of the penumbra. The sense of the note on that subject, which I have given, p. 313 (Phil. Trans. for 1792), is plain enough, that, as the sun is seen in Venus under an angle of 44′, the penumbra, assuming the diameter of Venus = 60″, can amount only to 0″,38 in the middle of her disc; but that as Venus, when her diameter is so large, can only appear under the phase of a crescent, the penumbra can scarcely amount to $\frac{1}{10}$ of a second in the perpendicular diameter on the line of the cusps. Instead of 0″,38, or still more accurately 0″,384, by an error of writing or computation 0″36 was set down: but what does this inconsiderable difference, *of* $\frac{1}{50}$ *sec.* impede in the conclusion, *that the penumbra at the boundary of light on the disc, or in the perpendicular direction on the line of the horns,* is imperceptible? and how could so unimportant a matter deserve the least notice?

3. *With respect to the twilight itself of Venus's atmosphere, and the computation of it,* the paper in question contains, p. 16 and 17, three objections: (*a*) that I had *overlooked the penumbra,* which, in the projection I have given of the crepuscule 15° 19′ is said to amount to *more than* 2°$\frac{1}{3}$, or, as this error of computation was corrected in my copy, to 1° 11′ 47″,6; (*b*) that my 7-feet speculum must be tarnished, *because I have measured the*

projected extent TOO SMALL; and (*c*) that *my calculations are so full of inaccuracies, that it would be necessary to go over them again, and compare them* EXACTLY WITH THOSE MADE BY MY OPPONENT.

It requires, indeed, little examination to perceive, that all these objections are groundless.

(*a*) That I did pay attention to the penumbra, my paper "on the atmosphere of Venus" shews plainly enough; and it is readily to be conceived, that the points of the horns, illuminated by refraction and penumbra, must project beyond the enlightened semicircle into the dark side. And it would also be easy to shew, how the points must project more beyond the enlightened semicircle, in proportion as the phase of Venus is that of a sharper crescent; with regard to which, I will hereafter determine, more accurately than my opponent has done, how much the projecting excess of the arch must be. But the author has not considered *that, in my way of making the measurement, it was quite unnecessary to take the penumbra into the computation*; for I measured the faint light, of a bluish-grey colour, which ran on along the edge of the dark hemisphere, according to fig. 20, (where A D indicates a diameter of Venus, parallel to the line of the horns) not, as he did 3 years after, from A, but only from B (the extreme visible point of the horn, still faintly illuminated by refraction and the diameter of the sun) to C; *and consequently I had, by the observation itself, already deducted the penumbra.* It is indeed possible, that at B and E, where the penumbra seemed to me to terminate, it yet might not be quite at an end; but the excess must be indefinitely small, since the *whole* projection of the penumbra,

from A to B, and from D to E, could not, by my calculation, amount to more than 0,63 second.

In general, such accuracy of computation avails nothing, *because the observations and measurements of such a very faint and always decreasing light, cannot be so very exact.* This is particularly shewn, and strikingly enough, in the two measurements of 20th May, 1793, given in the paper of my opponent; where the projection of this crepuscular light, taking the apparent diameter of Venus $= 60''$, was one time $12'',5$, and the other time only $7'',7$. And so much the more unimportant is it in the result of my calculation, that I assumed the crepuscular light as having been measured from A. But that in my way of measuring, in which the penumbra is abstracted by the observation itself, I have been happier and more accurate, is testified by the computations to be given presently of my two measurements of the years 1790 and 1793, which were made under different circumstances, and yet correspond uncommonly well.

(*b*) The second objection, *that I have measured the projection of the twilight too small*, is equally unfounded; for

(α) The projection found by the author must properly be somewhat larger than mine, because he did not, like me, measure the magnitude B C, but A C, fig. 20; and

(β) It will appear from the following computations, that I have found it AT LEAST AS LARGE as he did, without reckoning in the difference from A to B. He did not consider, that three years before I had observed under other circumstances, *which must make the extent of the crepuscule appear less;* and in general I do not perceive how he can form such a judgment from his

two measurements, which differ from one another so very much as $\frac{1}{3}$ of the whole magnitude. I can also assure him, that the 7-feet speculum, which I obtained in the year 1786 by his friendly kindness, has continued always so precious to me, that I have kept it in perfectly good condition to the present time.

As to

(*c*) the objection, *that my calculation abounds with inaccuracies,* it is indeed true, that the observation of March 12th, 1790, was not rigorously computed, yet *its exactness was carried much further than is necessary in observations of this kind;* for no one will comprehend the use of a scholastic computation to seconds and decimal parts of seconds, when the observations themselves leave an uncertainty of many minutes. However, to remove all doubt in this respect, and to save the author the trouble of a further careful comparison with his two measurements, I will here not only repeat the calculation in all its rigour, but also add the new one for my second measurement of the 21st May, 1793, and compare both together, as well as with that of my opponent.

(*a*) *Calculation of my observation of the 12th March* 1790, $6^h\ 0'$, *p. m.*

The time of this observation may be taken, without scruple, as $6^h\ 0'$ mean Paris time; for it was made after 6 o'clock at Lilienthal. The equation of time amounts to 10′, and the difference of meridians to 26′; therefore, if the observation had been made exactly at six, this would be $5^h\ 44'$ mean time at Paris.

Now, according to the latest tables by M. DE LA LANDE, we have, for that moment,

heliocentr. long. of Venus -	=	$5^s\ 18^\circ\ 41'\ 53''$
long. of the earth -	=	5 22 22 45
difference - -	=	3 40 52
heliocentr. latit. of Venus	=	3 23 6

therefore,

log. cos. $3^\circ\ 40'\ 52''$ -	9,9991030
log. cos. 3 23 6 -	9,9992416
	9,9983446

angle at the sun -	=	$4^\circ\ 59'\ 58''$
sum of the other angles -	=	175 0 2
half sum - -	=	87 30 1
log. of the distance of the earth from the sun	=	9,997766
log. of the distance of Venus from the sun	=	9,857040
log. tang. -		10,140726
	=	$54^\circ\ 7'\ 28''$
subtract		45 0 0
remain		9 7 28
log. tang. $9^\circ\ 7'\ 28''$ - -		9,205777
log. tang. 87 30 1 - -		11,359955
log. tang.		10,565732
half difference - -	=	$74^\circ\ 47'\ 42''$
half sum - -	=	87 30 1
angle at Venus -	=	162 17 43

angle at the earth = 12° 42′ 19″
compl. of the angle at Venus = 17 42 17

Now the crepuscular light of Venus, the measure being considered as a chord, extended 15° 19′; then consequently it is,

log. sin.	15° 19′ 0″ -	9,421857
log. sin.	17 42 17 -	9,483033
log. sin.		8,904890
		= 4° 36′ 28″

To so much, therefore, amounts the arch of a great circle, over which the crepuscule of Venus's atmosphere extends, as far as it can be distinguished on our earth, under favourable circumstances. According to my former computation, it came to 4° 38′ 30″: *wherefore the whole difference, certainly very inconsiderable to be given as an instance of inaccuracy, amounts* ONLY TO 2 MINUTES; and it is surely quite superfluous to include seconds in a calculation, which, from the circumstances of the observation, can only be depended on to several minutes.

If it be wished to take this opportunity of determining the arch, how far the points of the horns project on account of the apparent diameter of the sun seen from Venus, put the semidiameter of the sun seen from the earth at the abovementioned time, deducting 3″ for irradiation, = 16′ 3″,3 = 963″,3

log. 963,3 -	2,983762
log. dist. ☉	= 9,997766
	2,981528
log. dist. ♀ a ☉	9,857040

l. sin. d. ☉ *ex* ♀ -	3,124488	$= 1332'',0 = 22'\ 12''$
l. sin. 17° 42′ 17″	9,483033	
	3,641455	$= 4379'',8 = 1°\ 12'\ 59'',8.$

This is the quantity which, in the paper of my opponent, was erroneously stated at more than $2°\frac{1}{3}$, because the diameter was taken instead of the semidiameter; it was afterwards corrected to 1° 11′ 47″,6; but it is highly probable, that the points of the horns project still further, *on account of refraction.* However, as we do not know the quantity of the horizontal refraction on Venus, this cannot be ascertained with any certainty. It is sufficient for me, that I measured the crepuscular arch from the point where the extremity of the horn seemed to end in my instrument, and to my eye.

(β) *Calculation of my late observation of the* 21*st May*, 1793.

The time falls on 8^h 0′, mean Paris time; and therefore we have,

long. of the earth	=	8^s	1°	5′	55″
heliocentr. long. of ♀	=	7	27	13	27
difference	=		3	52	8
lat. of Venus -	=		1	1	37,6

log. cos. 3° 52′ 8″	-	9,9990091
log. cos. 1 1 37	-	9,9999302
log. cos. of the angle	-	9,9989393

angle at the sun	=	4°	0′	10″
sum of the other angles	=	175	59	50
half sum	=	87	59	55

log. dist. of the earth from ☉ = 0,005628
log. dist. of Venus from ☉ = 9,860276

log. tang. 10,145352
= 54° 24′ 50″
subtract 45 0 0

remain 9 24 50
log. tang. 9° 24′ 50″ - 9,219579
log. tang. 87 59 55 - 11,456615

log. tang. 10,676194
half difference = 78° ′5 53″
half sum - = 87 59 55

angle at Venus - = 166 5 48
angle at the earth = 9 54 2
compl. of the angle at ♀ = 13 54 12

Now, as I have stated above, the projected extent of the twilight measured 10″, putting the semidiameter of Venus = 30″; and, as I then measured that extension perpendicularly on the line of the cusps, these 10″ may be considered as a sine, and so the arch will amount to 19° 28′. Then

log. sin. 13° 54′ 12″ 9,380725
log. sin. 19 28 - 9,522781

8,903506
= 4° 35′ 34″

To so much, therefore, amounts, according to my *second*

observation, the arch of a great circle, over which the twilight of Venus's atmosphere extends, as far as we can discern it under favourable circumstances, and which we may put in comparison with our common twilight.

If this result be compared with that of my older observation of the 12th March, 1790, which was 4° 36′ 28″, it will be seen *that the two agree much more nearly than could have been expected in such delicate observations, namely, to the very inconsiderable difference of one minute;* and this is the more striking, as, according to the different situations of Venus, and the modifications of our own atmosphere, this crepuscular light is not likely to be ever observed, at different times, exactly of the same extent.

Great, however, as this agreement is, I am far from regarding it as any thing but a lucky accident. Whoever considers the manner of measuring, and the nature of the observed object, will be easily convinced, that we can never determine quite exactly the length of the twilight of Venus. The most accurate measurements of this kind admit errors of $\frac{1}{2}''$ in the projected extension; and this $\frac{1}{2}''$ alone would amount nearly to $\frac{1}{4}°$ in the computed arch of the great circle. Moreover the crepuscular light gradually decreases, and I only pretend to shew how far it continued visible, in my observation, with my eyes and instruments, under the state of the atmospheres of Venus and the earth at that time: the part which was thus visible to me extended, according to the computation given above, over something more than 4° $\frac{1}{2}$ of a great circle. But I am convinced that, under favourable circumstances of the weather, situation of Venus, and perfection of instruments, the atmosphere of Venus might possibly be traced something fur-

ther: this, however, has not been done, at least as yet; for if we compare with these measurements and calculations, which are certainly as accurate as I could make them,

(γ) *Dr.* HERSCHEL'S *observation of the 20th May,* 1793, when he measured the projection of the horns beyond a semicircle, in the evening likewise, about half past eight, *but a day earlier* than I did; it will be seen that he determines the magnitude of this projection on a mean from two measurements, *with the extreme exactness of* DECIMAL PARTS *of a second*, to be 18° 9′ 8″,2. But this mean is from two measurements which differ from each other, not barely by seconds or minutes, but by MANY DEGREES. In order to judge of the dependance to be placed on them, I will consider each of his measurements separately.

Ist measure.	log.	500	2,6989700
	log.	1195	3,0773679
			9,6216021
			= 24° 44′ 3″
IId measure.	log.	620	2,7923917
	log.	2400	3,3802112
			9,4121805
			= 14° 58′ 18″

His two measurements, therefore, give separately, the first 24° 44′, the second only 14° 58′. *An enormous difference of almost ten degrees,* which, according to my humble judgment, leaves the mean uncertain, not to seconds and their *decimal parts*, nor even to minutes, but properly to 5 degrees. It would therefore be useless to compare further with mine two examples,

which are so little exact, and agree so ill together; and I must leave it to be judged by others with what reason any person, from such inaccurate measurements, could consider mine as erroneous (which besides were made under other circumstances, in the year 1793), and the calculations founded on them as extremely inexact. Nevertheless, the mean deduced from those examples, namely, 18° 9′, agrees very well with my observation; for the following day, when the projection ought to be greater, I found it 18° 28′; though when it is considered that the penumbra must be deducted from the measurement of my opponent, the mean is somewhat too small. *His observation, therefore, by no means gives the extent of Venus's twilight greater than mine, but rather something less.*

Thus, by these new measurements and computations, the general results I have already deduced in my abovementioned paper " on the atmospheres of Venus and the moon," relative to the atmosphere of Venus, are still more confirmed and justified; and there is no longer any doubt, as my opponent agreeing with me allows, *that the atmosphere of this planet is very dense, like that of the earth.* Here then I might rest with regard to those conclusions; however, I find it useful to add the following explanations, in order to avoid further misunderstanding.

1. Although, according to those results, there is no doubt, that the atmosphere of Venus is as dense as that of our earth, yet I do not see in fact, from my observations, how we can confound, against all analogy, a general density, with particular, local and accidental, temporary modifications and condensations into clouds; and *so positively deny all transparency* to this atmosphere, as to assert *that in the shining of the planet we see by*

no means the light of its body, but merely that of its atmosphere.

Notwithstanding the density of this atmosphere, we must naturally consider it as generally clear and transparent, like our own, and that of the moon, and as losing its transparency only where its matter becomes really condensed; which condensations, however, may be supposed not always to appear like darker spots to an observer on our earth, but to remain often imperceptible to him. At least, I cannot think, contrary to all analogy, that Providence would bless the inhabitants of Venus, incomparably less than ourselves, with the happiness of seeing the works of almighty power, and of discovering, like a HERSCHEL, still more and more distant regions of the universe. We must, at least, adhere to this analogy, till indisputable experiments convince us of the contrary, which, however, according to my numerous observations, is by no means the case.

2. But if the atmosphere of Venus be naturally clear and transparent, like that of our earth, except accidental condensations, we cannot well doubt, that in looking at the planet, we perceive at the same time both the light of its body, and that of its atmosphere, the latter being illuminated partly by the immediate rays of the sun, and partly by reflection from the body of the planet, and by refraction.

3. It is also equally reasonable to suppose, that, as we are ourselves enveloped in a thick atmosphere, and must look, from a great distance, through a dense illuminated atmosphere, not only our own atmosphere, but likewise particularly the density of that of Venus, and the light upon it, as also the various reflections of the light from the body of the planet, and its refractions, will put such impediments in the way, and occasion

such indistinctness, *that we never can distinguish, as we do in the moon, a projection of the land on the surface of the planet, nor even the shadows cast by its mountainous inequalities, unless it be under a combination of every favourable circumstance, and even then only in a faint undefined manner.* This will be more readily apprehended, when we consider, that the shadows on Venus must appear, from the density of her atmosphere, and its reflection and refraction of light, only *dark-grey*, like those on the earth, and not *black*, as they are on the moon.

4. Yet, in the same manner as in the moon, we discern in Venus, even under the most favourable circumstances, only those parts of her surface, which lie nearest to the boundary of illumination, at the time when we see her half enlightened, because then we look, in a shorter line, perpendicularly through her atmosphere, and moreover the reflection and refraction are much less injurious, and the shadows are longest. Only at such times, and when the atmosphere is likewise clear over such parts of her surface, can we see these shadows, which do not appear sharply terminated, but like a faint mixture of greyish shade and light, sensible enough, but not clear.

5. Granting this rational theory, so conformable at least to our experience on this earth, and to analogy, all the phænomena I have pointed out are very easily and clearly explained by it; and this experience shews at the same time the justness of the theory, and that it cannot well be otherwise.

Thus we can naturally account for,

(*a*) The soft mixture of light and shade, to be seen only near the time of the greatest elongations, *yet not always, but only sometimes,* and at those moments alone when the atmo-

sphere there, and our own, are favourable for the purpose: to this belong also the shadows sometimes seen by me at the southern horn, and which separated the extreme point of it wholly, or in part. It is possible likewise, that the atmosphere may be clear in one place alone of the boundary of light, in which case we should see something of a shadow there only, the boundary line appearing in the other parts as usual, not streaked with shade, but only not sharply terminated: so, for instance, it was on the 31st of July last year, when Dr. Olbers observed here with me.

(*b*) But if Venus be considerably more or less than half enlightened, the shadows are not only shorter in themselves, and less perceptible in so small an image, but likewise we see them obliquely, and in a sensibly longer line through the illuminated atmosphere of the planet, which then covering the shadows more, renders them more difficult to be distinguished, and commonly quite invisible. It is, therefore, useless to expect such appearances of shadow, in small crescent phases of Venus, although she be then vastly nearer, and her apparent diameter much larger. If there are at those times real shadows on her, we see the places, not as spots of shade, but as indentations; and to this belongs the remarkable observation, when the boundary arch of light appears irregular, sometimes in larger and sometimes in smaller parts, and the point of one horn, nay even a considerable part of the horn, is evidently slenderer than the other.

Here it will be readily understood,

(*c*) That as our own atmosphere has an influence on the distinctness of all such phænomena, so accidental condensations in the atmosphere of Venus may cause many bright parts,

not lying in the shade, to assume the appearance of dark spots. This accident, however, of which indeed I have no sufficiently certain experience, must occur but seldom, because I have hitherto perceived the mixture of shade and indentation only at the boundary of light; and it would not be easily explained, *why those dark places should not be perceived further in upon the enlightened parts, unless they were true shadows of mountains, and not barely atmospherical appearances.*

Thus at least is every thing to be explained very naturally; and if the phænomena themselves are put out of all doubt by me and others, they confirm the propositions delivered above. And equally insignificant appears to me also, the doubt which

6. A phænomenon might raise, that occurred to my opponent only or chiefly in April of last year: the same, as may easily be supposed, was seen by me many years ago, but especially in 1790, and frequently since; though, not thinking it particularly instructive or remarkable, I forgot to deliver it separately in my paper "on the atmospheres of Venus and the moon."

The phænomenon in question, according to my older observations, consists in this; that the external edge, for a very small breadth, appears incomparably brighter than the rest of the enlightened part, nearer to the boundary of light; and forms a much brighter small border, which is sharply terminated at its outer edge, but on its inner side appears without any sharp boundary, losing itself in the weak light of the rest of the illuminated part; so that in general, the falling off, or gradual diminution, of the light toward the line bounding the illumination, *is perceived according to the photometrical laws, but particularly becomes chiefly striking nearer to the boundary of light.*

What seems to deserve further attention in this phænomenon, is the circumstance, *that I have seen this extremely brighter border at the edge, not only about the time of Venus's greatest digressions from the sun, when she appears to us half enlightened, or more, but also equally well very near the conjunction; and particularly plain in the year 1790, when she had the very smallest crescent phase, not amounting to more than from 4 to 6 seconds in breadth.*

Were it not for this remarkable circumstance, I should look for the cause solely in the greater quantity of light, which, when the planet has the phase of being half, or almost half illuminated, falls quite or nearly perpendicular through its atmosphere, on the surface which appears to us the edge, and is reflected back from this surface into the atmosphere, by which it is again reflected, and in various ways refracted, so that at the edge, against which we look by an oblique long line through the atmosphere, we see an exceeding quantity of light, *being that of the planet and its atmosphere at the same time;* but the abovementioned observation seemed to make it probable, that, as I have always believed, the appearance chiefly depends on optical fallacy, yet this still requires further investigation. However, though we are as little acquainted with the natural constitution of the ball of the planet, in respect to its power of reflecting more or less light, as with the species of the refraction there, yet it seems contrary to all analogy, that the atmosphere of this heavenly body should be an *opaque* cover, capable of reflecting more light than the solid body itself; yet that we *should see the external edge, not faintly expressed, in the manner of an atmosphere, but sharply terminated; and, on the other hand, the boundary of light, under*

favourable circumstances, streaked with shade, exhibiting an irregular arch of termination, with indented spots, unequal horns, and so forth. I shall, therefore, at least till adequate reasons convince me otherwise, never assent to a bare hypothesis, that in this planet we merely see its atmospherical cover, and never the body itself; unless when, very rarely, a clearing up of its atmosphere allows us to get sight of a small part of its real surface, in the dark form of a cloud-like spot.

Finally, as to what the celebrated author has remarked besides, *on the apparent diameter of Venus, in the mean distance of the earth;* namely, that by a mean of the measurements he made Nov. 24th, 1791, with the 20-feet reflector, it amounts with great certainty, to 18″,79; and that therefore the planet is larger than it has been given by astronomers hitherto: this is a matter which belongs only indirectly to my object here.

I could have wished that he had not depended too much on a single instrument, having an excess of light, in which the irradiation may unobservedly extend further than in weaker telescopes, nor on a single micrometer; but had reduced his mean from many measurements, made with various and less powerful telescopes, and on many days, under very different apparent diameters, in order to his conclusion for the mean distance of the earth; because, as I only observe here previously, for want of room, I doubt very much of the dependence to be placed on those measures; and must consider this, at least, as rather too large, until I can convince myself of the contrary.

Comparing this determination with that which has been

adopted hitherto, according to M. DE LA LANDE, namely 16″,7, it follows by calculation, that on the 12th March, 1790, when I found the apparent diameter 59 to 60 seconds, it should have been, by M. DE LA LANDE, 58″,58, but by this new determination, 65″,91; and on the 21st May, 1793, when I found it greater in proportion, probably because the planet was lower, and had therefore more irradiation, namely 60 seconds, it should by M. DE LA LANDE have been only 56″,75, but by the new determination 63″,85; consequently, according to the latter, I must have overlooked 4 seconds on the 21st May, 1793, and on the 12th March, 1790, when Venus appeared to my eye particularly distinct, *fully 6 seconds.* Both, and especially the last, seem to me contrary to all probability.

As the author, since the year 1780, has measured the diameter 7 different days, so have I before me no less *than* 24 *different* measurements, made since the year 1788 only: in these I took the apparent diameter of Venus, sometimes when she was at a greater, and sometimes at a less distance; not only repeating the measurement each time, but often 6, 7, or more times, with different telescopes, magnifying powers, and projection micrometers. If, out of so considerable a number of observations, the mean of the measurements made at each time be taken, and reduced to the mean distance of the earth from the sun, and then the mean of all these reductions be found, this must give the apparent diameter of Venus, at the mean distance, as exactly as possible. Having so great a number of measurements, I must reserve this subject for a particular memoir: yet I think it my duty previously to announce, that in so many observations, I have always found

her apparent diameter agree, to 1 or 2 seconds, with that given in the Ephemerides for the time; and as these are computed on the determination hitherto adopted, of 16″,7, we may continue to reckon Venus of about the same size as she has hitherto been estimated.

Lilienthal,
April 1, 1794.

Fig. 1.

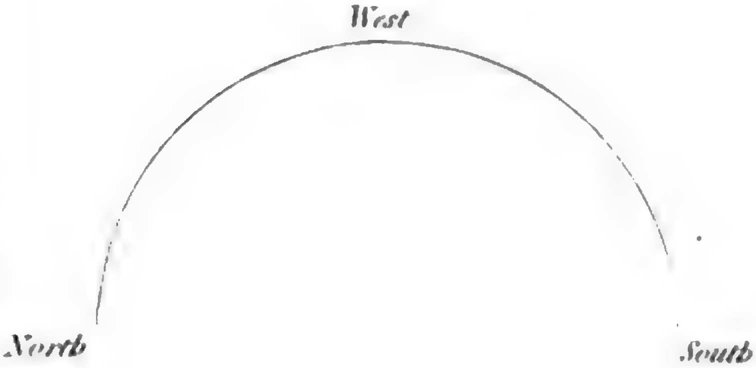


Fig. 2.

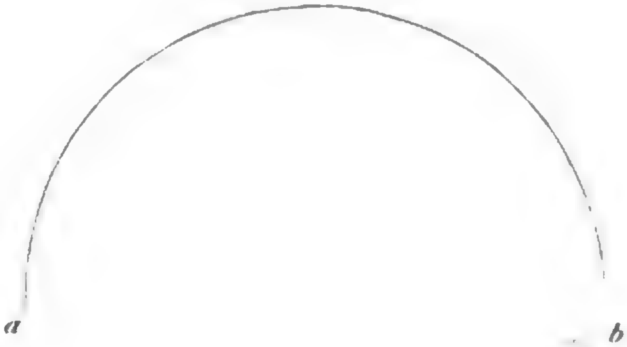


Fig. 3.

Fig. 4.

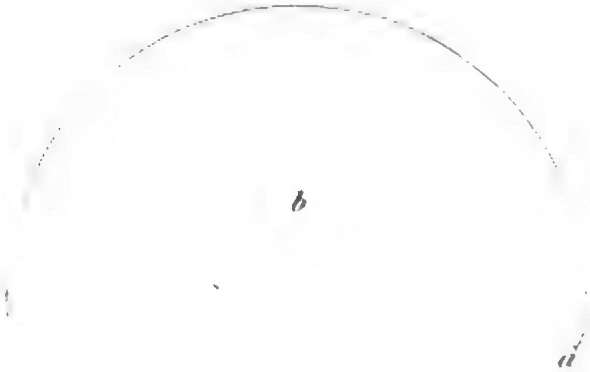


Fig. 5.

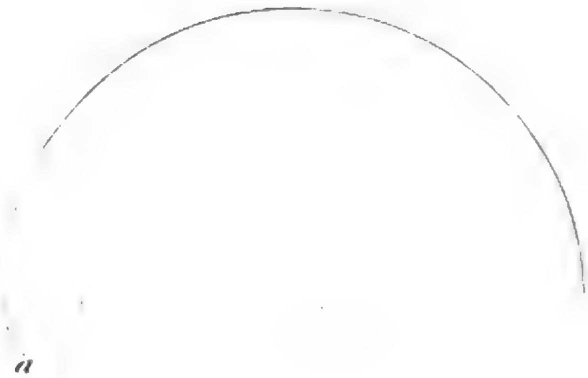


Fig. 6.

Fig. 7.

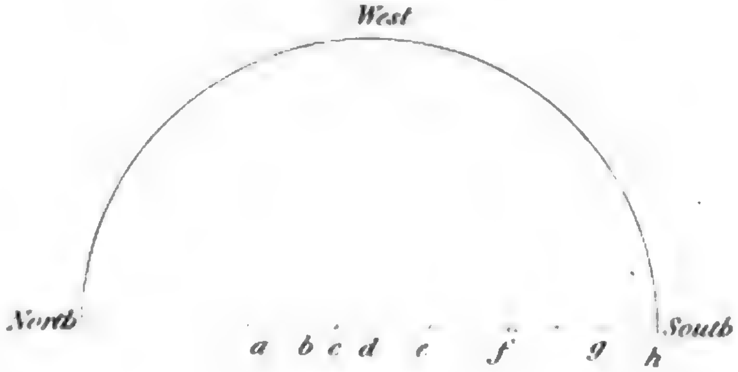


Fig. 8.

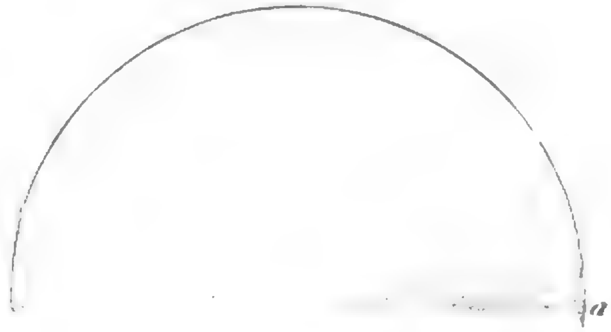


Fig. 9.

Fig. 10.

Fig. 11.

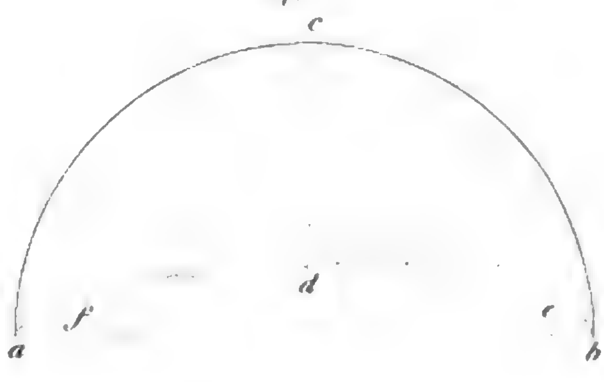


Fig. 12.

Fig. 13.

Fig. 14.

Fig. 15.

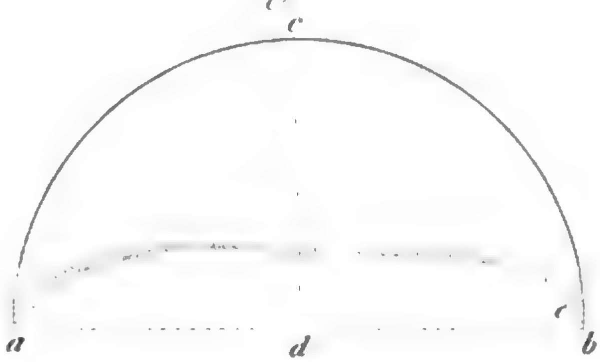


Fig. 16.

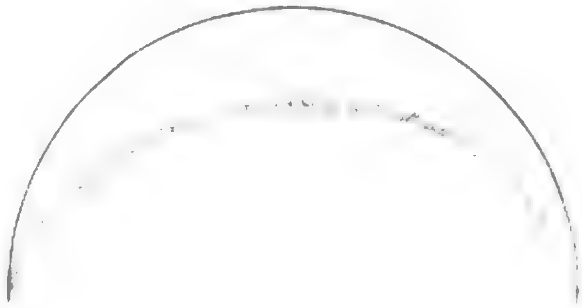


Fig. 17.

Fig. 18.

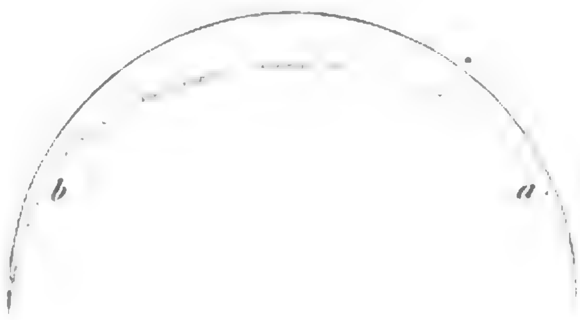


Fig. 19.

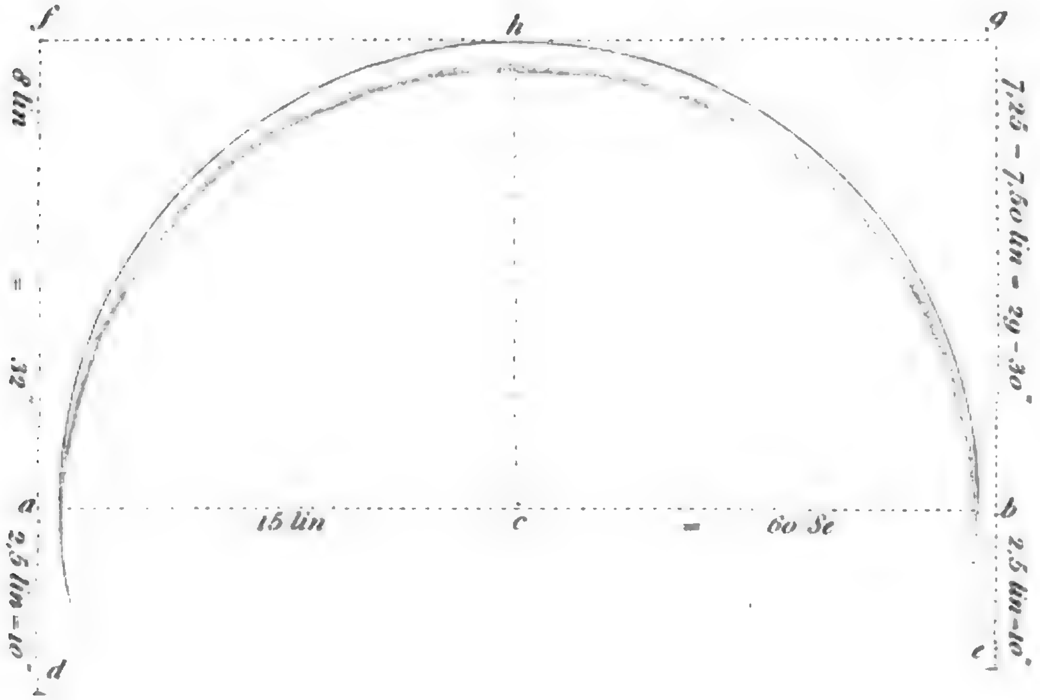
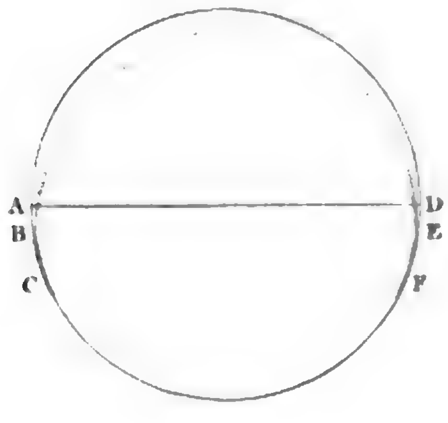

f
h
g
8 lin = 32°
7,25 - 7,50 lin = 29 - 30°
a
15 lin
c
= 60 Se
b
2,5 lin = 10°
2,5 lin = 10°
d
e

Fig. 20.


A
B
C
D
E
F

VI. *Experiments on the Nerves, particularly on their Reproduction; and on the Spinal Marrow of living Animals. By* William Cruikshank, *Esq. Communicated by the late* John Hunter, *Esq. F. R. S.*

Read June 13, 1776.

THE nerves on which these experiments were made are, the par vagum, and intercostal. The par vagum arise from the basis of the brain, pass through the basis of the skull, along with the internal jugular veins. They are distributed to the tongue, œsophagus, larynx, heart, and lungs; and, running on each side of the œsophagus, may be said to terminate in the stomach, liver, and semilunar ganglion of the intercostals, below the diaphragm; from whence they are again distributed to the viscera of the abdomen. The intercostals also arise from the basis of the brain, pass through the basis of the skull, along with the carotid arteries. They at first run by the fore part of the vertebræ of the neck, still adhering to the coats of these arteries; but having reached the chest, they leave these arteries, and run before the heads of the ribs, where, sending off branches which pass between the ribs, they have thence been named intercostals. Several of these branches uniting, form a trunk on each side, which, running forwards towards the middle of the spine, perforates the diaphragm, and then terminates in the semilunar ganglion of the intercostals. These trunks are distinguished by the name of the anterior intercos-

tals. The original trunks continue their course by the sides of the lumbar vertebræ; after which, they run before the os sacrum, and, approaching nearer each other as they descend, terminate before the os coccygis, in the ganglion coccygeum impar of WALTHER. Their branches all go to the heart, abdominal viscera, testicles in men, and ovaria and uterus in women. The trunks of these nerves are largest in the neck. In the human species, the two nerves of each side are distinct; but in those quadrupeds which I have examined, they are so closely connected through the whole length of the neck, as to make apparently but one nerve. The intercostal is the smallest nerve, and adheres so closely to the other, as to be with difficulty separated from it. They seem to me, likewise, larger in the dog, compared with his bulk, than in the human subject. The neck was the place in which I chose to divide these nerves; it was there they could be got at with least danger, a circumstance which, by making an experiment more simple, makes it consequently more to be relied on; and, in order to put the animal to as little pain as possible, and make the operations short, I chose to divide both nerves at once, rather than take up time in separating them, and dividing them singly; so that, instead of four operations on each animal, I confined myself to two. Instead of mentioning the names of the gentlemen present at each experiment, I shall observe once for all, that two or more of the following gentlemen were present at each experiment, except experiment VII, which I performed, assisted by Mr. HUNTER's servant only:—Messrs. BARFORTH, BAYLEY, DAVIDSON, HARTLEY, HAWKINS, HOME, KUHN, NOBLE, PARRY, MARTIN, SHELDON, WHEATLY; besides others, who came in occasionally, during the time of the experiments, or who

afterwards saw the animals, while the described symptoms were taking place.

EXPERIMENT I.

January 24th, 1776, I divided, in a dog, one nerve of the par vagum, with the intercostal, on the right side. The symptoms, consequent to the operation, were heaviness, and slight inflammation of the right eye; breathing with a kind of struggle, as if something stuck in his throat, which he wanted to get up; sullenness, and a disposition to keep quiet: the pulse did not seem much affected, nor had he lost his voice in the least. The unfavourable symptoms did not continue above a day or two; and on the eighth day he was in very high spirits, and seemed perfectly to have recovered.

EXPERIMENT II.

February 3d, I cut out a portion of the two nerves of the opposite side, in the same dog; the piece might be about an inch long. His eyes became instantly red and heavy; his breathing was more difficult than in the former experiment; he was sick, and vomited frequently; the saliva was increased in quantity, and flowed ropy from his mouth; his pulse in the groin was about 160 in a minute; he ate and drank, however, even voraciously at times, and had stools; he never attempted to bark or howl, probably because he did not feel great pain; and yet his attention was not so much disengaged from internal uneasiness, as to be excited with ordinary causes from without; in breathing, the inspirations were slow and deep;

the expirations were attended with repeated jerks of the abdominal muscles, as if he wanted more effectually to expel what air was contained in the lungs. The seventh day after this second operation, he was found dead, at a considerable distance from his bed. In the dead body, every thing seemed in a sound state, except the lungs: these contained little or no air; in consequence of which, they sunk to the bottom in water; they were of a red brown colour, resembling more the substance of a sound liver, than that of inflamed lungs. The inner surface of the trachea and its branches was exceedingly inflamed, and covered with a white fluid, in some places resembling pus, in others ropy, and more of the nature of mucus. The divided nerves of the right side were united by a substance of the same colour as nerve, but not fibrous; and the extremities formed by the division were still distinguished by swellings, rounded in form of ganglions. The same appearance had taken place, with respect to the nerves of the left side; though the divided extremities seemed to have been full two inches apart; the uniting substance was more bloody than that of the other side. This experiment was made, to prove that the original power of action in the thoracic and abdominal viscera was independent of the nerves. As I found the nerves regenerated, a circumstance never hitherto observed, it occurred to me, that it might be objected to the reasoning, that the two first nerves were doing their office, before the two last were divided: to obviate this objection, I made the following experiment.

EXPERIMENT III.

February 19th, I divided, at one operation, the four nerves

composing the first class, in a dog. His eyes became instantly dull and heavy; he tottered as he walked; foamed at the mouth; vomited two or three times; breathed with excessive difficulty; his inspirations were long and deep, his expirations short and sudden, but not attended with the repeated jerks of the abdominal muscles as in the last animal; he barked loud every time he threw out the inspired air from the lungs; the pulse was quicker than before the operation. Next morning about half after eight, I found him apparently dead; but on examining more attentively, found he breathed still, though exceedingly slow; his pulse was gone, and he felt cold; his limbs were stretched out. On placing him near the fire, he began in a few minutes to breathe distinctly, and the heart now and then gave a pulsation; in about four hours, he seemed to have got to the same state the operation first left him in, and barked at every expiration, his pulse beating then fifty in a minute. About four in the afternoon he died, having survived the operation twenty-eight hours. The lungs in the dead body were found loaded with blood, but not so much as to carry them to the bottom in water. The trachea was not inflamed. The nerves of the right side, from which a portion had been cut out, seemed to have undergone little alteration; they were only a little more vascular than usual, and had the rounded swell where they had been divided. The nerves of the left side, which had retracted but little, and had been only divided, had their extremities covered with a plug of coagulable lymph. I suspected that the reason of the first dog's dying so soon, was, that none of the nerves had yet acquired the power of performing their former offices; and that, were the operations performed at a greater distance of time, the

animal would recover. With this idea, I was led to repeat my experiments, allowing a greater interval to take place between the first and second.

EXPERIMENT IV.

March 6th, I repeated experiment I. on a large dog. His eye on the right side seemed instantly affected, looked dull and inflamed; he coughed and breathed with some difficulty; the secretions from the salivary glands were much increased; he had tremors; these, however, I attributed partly to fear, as on caressing him they disappeared. He ate and drank very well, and had stools. Most of these symptoms continued but a few days, the eye becoming more clear, and the difficulty of breathing hardly perceptible; he vomited, but only after eating, a circumstance which often takes place in dogs in perfect health, from devouring their food too greedily. Thus he continued for three weeks; the external wound had healed, almost by the first intention; he ate greedily, and had perfectly recovered: I supposed the regenerated nerves might now be performing their offices.

EXPERIMENT V.

March 27th, I repeated experiment II. on the same dog, but did not remove quite so much of the nerves. He was stupid for a minute or two, and gaped for breath; but in a few minutes more these symptoms went off; in a quarter of an hour after he ate some boiled meat, with his usual avidity; all the symptoms of the preceding operation again took place,

and in the same order. The vomiting and difficulty of breathing were rather more considerable; he ate and drank notwithstanding, and had stools. The convulsive jerks of the abdominal muscles, which hardly took place in the last experiment, were observed in this, during expiration, but were not constant, as in the first dog. On the 15th of April he was nearly as well as before the operations, only he was leaner, and perhaps weaker, from the confinement, as well as from the operations. I wished to see the state of the nerves; an artery was opened in the groin, and the animal expired in a few seconds. In examining the dead body, the viscera were all, to appearance, sound. The divided nerves of the right side were firmly united; having their extremities covered with a kind of callous substance; the regenerating nerve, like bone in the same situation, converting the whole of the surrounding extravasated blood into its own substance. The nerves of the left side were also perfectly united; but the quantity of extravasated blood having been less, the regenerated nerves were smaller than the original; I observed too, that they did not seem fibrous like original nerves, but the recollection that the callus of bone is dissimilar to the original bone, quieted whatever doubts could arise from this circumstance. The tonsils were considerably inflamed, and this circumstance alone might be sufficient to account for the increased secretion of the saliva, an attendant symptom of most sore throats; though I have also seen an increase of viscid saliva, in the human species, from hypochondriac affections of the digestive powers, and also from the causes of temporary debility. The regeneration of the nerves which took place in the first dog, and which I

think fully proved by this experiment, was a circumstance to me, then, unexpected and unthought of.

EXPERIMENT VI.

April 19th, I divided the spinal marrow of a dog, between the last vertebra of the neck and first of the back. The muscles of the trunk of the body, but particularly those of the hind legs, appeared instantly relaxed; the legs continued supple, like those of an animal killed by electricity. The heart, on performing the operation, ceased for a stroke or two, then went on slow and full, and in about a quarter of an hour after, the pulse was 160 in a minute. Respiration was performed by means of the diaphragm only, which acted very strongly for some hours. The operation was performed about a quarter of an hour before twelve at noon; about four in the afternoon the pulse was ninety only in a minute, and the heat of the body exceedingly abated, the diaphragm acting strongly, but irregularly. About seven in the evening, the pulse was not above twenty in a minute, the diaphragm acting strongly, but in repeated jerks. Between twelve at night and one in the morning, the dog was still alive; respiration was very slow, but the diaphragm still acted with considerable force. Early in the morning he was found dead. This operation I performed from the suggestion of Mr. HUNTER: he had observed in the human subject, that when the neck was broke at the lower part, (in which cases the spinal marrow is torn through), the patient lived for some days, breathing by the diaphragm. This experiment showed, that dividing the spinal marrow at this place

on the neck, if below the origin of the phrenic nerves, would not, for many hours after, destroy the animal; it was preparatory to the following experiment.

EXPERIMENT VII.

April 26th, I divided all the nerves of the first class, in a dog. The principal symptoms of experiment III. took place. Soon after, I performed on the same animal the operation of experiment VI.; the symptoms peculiar to this operation also took place, whilst those peculiar to experiment III. disappeared. His respirations were five in a minute, and more regular than in experiment III.; the pulse beat 80 in a minute. Five minutes after, I found the pulse 120 in a minute, respiration unaltered; at the end of ten minutes the pulse had again sunk to 80 in a minute, respiration as before. At the end of fifteen minutes, the pulse was again 120, respiration not altered. The operation was performed about two in the afternoon, at Mr. HUNTER's, in Jermyn-street. At three quarters of an hour after five, the respirations were increased to fifteen in a minute; the pulse beating 80 in the same time, and very regularly; the breathing seemed so free, that he had the appearance of a dog asleep. At a quarter before eight, the pulse beat 80, respirations being ten in a minute. At three quarters of an hour after ten, respiration was eight in a minute, the pulse beating 60. The animal heat was exceedingly abated: I applied heat to the chest, he breathed stronger, and raised his head a little, as if awaking from sleep. At half after twelve, Mr. HUNTER saw him; the breathing was strong, and twelve in a minute, the heart beating forty-eight in the

same time, slow, but not feeble. He shut his eyelids when they were touched; shut his mouth on its being opened; he raised his head a little, but as he had not the use of the muscles which fix the chest, he did it with a jerk. Mr. HUNTER saw him again between four and five o'clock in the morning; his respirations were then five in a minute, the heart beating exceeding slow and weak. We suppose he died about six in the morning, having survived the operation sixteen hours. This experiment I made from the suggestion of Mr. HUNTER, with a view to obviate the objections raised against the reasoning drawn from the three first experiments. It was urged, that though by these experiments I had deprived the thoracic and abdominal viscera of their ordinary connection with the brain, yet, as the intercostals communicated with all the spinal nerves, some influence might be derived from the brain in this way. This experiment removed also the spinal nerves, and consequently this objection.

As I found, by the two last experiments, that dividing the spinal marrow in the lower part of the neck did not immediately kill, although instant death was universally known to be the consequence of dividing it in the upper part of the neck, I expressed my surprise to Mr. HUNTER, that the spinal marrow should, according to modern theory, be so irritable in the one place, and so much less so in the other.

He told me, that from the time he first observed, that men who had the spinal marrow destroyed in the lower part of the neck lived some days after it, he had established an opinion, that animals, who had the spinal marrow wounded in the upper part of the neck, did not die from the mere wound; but that in dividing it so high, we destroyed all the nerves of the

muscles of respiration, and reduced the animal to the state of one hanged; whereas in dividing it lower, we still left the phrenic nerves, and allowed the animal to breathe by his diaphragm. If this opinion be well founded, though dividing the spinal marrow in the lower part of the neck does not kill instantly, whilst the phrenic nerves are untouched; yet if I divide the phrenic nerves first, and then divide the spinal marrow in the lower part of the neck, the consequence, I said, will be the same, as if I had divided it in the upper part.

EXPERIMENT VIII.

By detaching the scapulæ of a dog from the spine, and partly from the ribs, I got at the axillary plexus of nerves, on both sides, from behind. I separated the arteries and veins from the nerves, and passed a ligature under the nerves, close to the spine. I thought I could discern the phrenic nerves, and instantly divided two considerable nerves going off from each plexus. The action of the diaphragm seemed to cease, and the abdominal muscles became fixed, as if they had been arrested in expiration, the belly appearing contracted. His respirations were now about twenty-five in a minute, the pulse beating a hundred and twenty. As I was not willing to trust the experiment to the possibility of having divided only one of the phrenics (which I afterwards found was really the case), and some different nerve instead of the other, after carefully attending to the present symptoms, I divided all the nerves of the axillary plexus, of each side. The ribs were now more elevated in inspiration than before; respirations were increased to forty in a minute; the pulse still beating

a hundred and twenty in the same time. Finding that respiration went on very easily without the diaphragm, in about a quarter of an hour after dividing the axillary plexus of each side, I divided the spinal marrow, as in experiment VI. The whole animal took the alarm, all the flexor muscles of the body seemed to contract, and instantly to relax again; he died as suddenly, as if the spinal marrow had been divided in the upper part of the neck. I then opened the chest, and found the heart had ceased its motion; I immediately introduced a large blowpipe into the trachea, below the cricoid cartilage, and inflating the lungs, imitated respiration. The heart began to move again, and in about three minutes was beating seventy in a minute. I recollected that there was still a communication between the brain, and the thoracic and abdominal viscera, that the par vagum and intercostals were entire, and turning to the carotids, divided the nerves. I then went on inflating the lungs as before; the heart, which had stopped, began to move again, beat seventy in a minute, and continued so for near half an hour after the animal had seemingly expired. These appearances were not confined to the neighbourhood of the heart; one of the gentlemen who assisted me, cried out once, that he felt the pulse in the groin. I now ceased to inflate the lungs, and presuming that I could easily reproduce the heart's action, allowed three minutes to elapse. On returning to inflate the lungs, I found the heart had now lost all power of moving; and that irritating the external surface with the point of a knife, did not produce the smallest vibration. I then irritated the phrenic nerves with the point of a knife; the diaphragm contracted strongly as often as the nerves were irritated. I irritated the stomach

and intestines, which also renewed their peristaltic motions. I then irritated the par vagum and intercostals, about an inch above the lower cervical ganglion of the intercostal; the œsophagus contracted strongly through its whole length, but the heart continued perfectly motionless. On dissection, I found a small branch of a nerve, running down from the second cervical to join the phrenic of the right side, but too insignificant to have any effect on the experiment. This experiment confirms those made by Mr. HUNTER, in which he recovered the animals by inflating the lungs, and on which his method of recovering apparently drowned people principally rests. It shews that respiration is the prime mover of the machine, and it takes off whatever objections might have been raised, from the animals, upon which he made his experiments, having the connection with the brain entire (as the par vagum and intercostals were not divided), since here the same thing took place in these experiments where nerves could have no effect.

If, in the opinion of the judicious, these experiments have a tendency to be useful to mankind, the author will forgive those censures, which unphilosophic severity may throw on him, whilst it views, only, some unavoidable circumstances attending the performance of them.

EXPLANATION OF THE PLATE (Tab. XVI.)

Fig. 1. shows the trachea, par vagum, and intercostals of the subject of experiments I. and II; the transverse bristles show the quantity of nerve lost by excision, and of course the quantity gained by regeneration.

Fig. 2. shows the same parts in the subject of experiment III: The bristles point out the mode of reunion of the divided nerves by coagulated blood.

Fig. 3. shows both the complete reunion of the nerve after division, and its regeneration after the loss of substance, in the subject of experiments IV. and V.

VII. *An experimental Inquiry concerning the Reproduction of Nerves. By* John Haighton, *M. D. Communicated by* Maxwell Garthshore, *M. D. F. R. S.*

Read February 26, 1795.

An animate machine differs from an inanimate one in nothing more conspicuously, than in its power of repairing its injuries, and of curing its diseases.

It is wisely contrived by nature that, in many instances, the cause producing the injury lays the foundation for the cure; for as injuries, particularly those occasioned by cutting instruments, are necessarily attended with an effusion of blood, from the division of blood-vessels, this fluid, either immediately or remotely, fills up the breach. Hence every part possessed of vascularity, and consequently of blood, carries with it the principle by which it repairs its injuries; and the facility with which this process is conducted, generally bears some proportion to the freedom of the circulation in each individual part.

But it has been a subject of inquiry with anatomists and physiologists, to determine of what nature the new formed part is, and how far it may be said to possess the characters of the original part. There are few who will deny, that a bone, when fractured, fills up the chasm with a substance of its own kind; or that a tendon, when divided, repairs with a substance resembling itself. But this law of nature is not admitted as univer-

sal; and this power of repairing in kind has been denied to several of the constituent parts of an animal machine. With respect to the nerves, it has been both affirmed and denied: some assert, that the new formed substance possesses the characters of the primitive nerve; others maintain, that it is totally different; and both found their opinions on experiment.

When opinions so opposite to each other prevail on a point, which experiment seems so fully adequate to decide, we are naturally led to take a view of the manner in which the experiments were conducted, and consider the criterion to which each party appealed.*

There are only two tests which seem to offer themselves, and from which any degree of judgment can be formed. These are, either a minute and careful examination of the new formed substance in an anatomical way, and an accurate comparison of it with the original nerve; or, a cautious attention to the function of that nerve, by which we see the loss of it from the division, and the return of it from the reunion of the divided parts.

Those who have subjected this matter to the test of experiment, have made their appeal to the first criterion; and have either affirmed or denied the reproduction, according as they thought the new formed part either agreed with or differed from the original nerve.

This criterion certainly supposes, that anatomy is fully competent to determine, what is the precise structure of nerves, what are the nature and characters of ultimate nervous fibres, and by what mechanism or power they execute their allotted

* Vide FONTANA, and ARNEMANN.

function. It supposes likewise (and which by the way is not true), that anatomists are perfectly agreed upon this matter; and that those who make their appeal to anatomy, have admitted a common standard of comparison, by which they allow their experiments to be judged; but no position is more remote from fact. It is sufficient to say, that some think ultimate nervous fibres are constructed to act by tremors, whilst others believe them to be hollow tubes. Nor is the difference of opinion less, respecting the appearances which they exhibit on being viewed by a microscope. One eminent physiologist* observes, that the ultimate nervous fibres are " serpentine and convoluted, " very much resembling the winding of the seminal ducts in " the testicle, or epididymis:" but having extended his microscopical observations to other parts, he finds a similar disposition of fibre; nay, even neutral salts, in a state of crystallization, and metals, when microscopically examined, have convoluted fibrous appearances, corresponding with those of nerves. Another ingenious inquirer,† having subjected the nerves to microscopic examination, thought at one time that their fibres were composed of cylinders, with bands twined around them, in a spiral direction; but subsequent examinations convinced him, that this appearance had its origin in an optical deception, and that their true direction was that of " parallel winding fibres." I have not yet heard whether a third examination has rectified the errors of the two former.

As it appears then, that microscopical observers neither agree with each other on this subject, nor with themselves, I think it fair to conclude, that ocular inspection cannot be admitted as a fair appeal, from which we can determine whether

* Dr. MONRO. † FONTANA.

the substance which unites the extremities of divided nerves is of the same nature as the original nerve.

Dr. ARNEMANN, of Gottingen, who has written *ex professo* on the reproduction of nerves, denies positively, from anatomical examination, that the new formed substance is of the nature of nerve; and on being shown the result of some of my experiments, he declared at the first glance of the eye. "that the medium of union did not possess the characters of "nerve;" and further, "that the true nervous substance is "never reproduced." But he had already prejudged the matter. On the other hand, I am persuaded that if the same preparations had been shown to the Abbé FONTANA, he would have seen in the new formed substance a continuation of the winding parallel fibres, agreeable to the result of his own experiments.

Such a contrariety of opinions determined me to decline an appeal so undecisive, and to submit my inquiries to a test less doubtful and fallacious: and as such a test was not to be found within the pale of anatomy, I resolved to try whether the resources of physiology could not furnish me with what I wished.

From physiology we learn, that *if the action of a nerve be suspended by a division of it, and if that action be recovered in consequence of an union of its divided extremities, such medium of union must possess the characters and properties of nerve.* I had therefore only to determine, what nerves appeared the most favourable for the experiment, and pursue the position just stated to its ultimate consequence. I know not whether my choice was judicious, but I determined on the eighth pair.

The first step I took in this inquiry, was to ascertain *what effects will arise from the division of both of these nerves, together*

with that branch of the great sympathetic nerve accompanying and strongly adhering to them.

EXPERIMENT.

A dog being properly secured, and a convenient incision made on the fore part of the neck, I divided both the nerves of the eighth pair: he became immediately restless and uneasy, betraying symptoms of great distress upon the stomach, which continued eight hours, when he died.

Though the result of this experiment is perfectly agreeable to what other experimental physiologists have stated, I thought it of importance to the present inquiry, to give it confirmation by further experiment. I therefore repeated it on two other dogs, one of which survived it three days, the other only two.

From these experiments we learn, that the action of these nerves was suspended, and that those vital organs which received their nervous energy from this source, had their functions arrested, so that death followed as a necessary consequence.

It may be said here, by way of objection, that a violent shock had been suddenly given to the machine; and that the animal perished rather from the sudden deprivation of the nervous influence, than from its absolute loss; and that if the same quantity had been abstracted in a more gradual way, the animal might have survived it. How little validity there would be in such an objection, the following experiment will evince.

EXPERIMENT.

Another dog being procured, I divided only one of the nerves of the eighth pair. I was surprised to see how slightly

he was affected from it; for, excepting a little moroseness, there was scarcely any alteration perceptible, so that in a few hours after the operation he took food as usual. On the third day, I divided the other nerve; but the same symptoms immediately supervened here as followed the division of both nerves in the former experiments: he continued in a state of restlessness and anxiety, with palpitations and tremors, until the fourth day, when he died.

The event of this experiment differs in nothing from the former, than that the fate of the animal was suspended a little longer, but the ultimate effect was exactly the same: therefore, in the first experiments, *the death of the animal is not to be imputed to the mere sudden deprivation of nervous energy, but to its absolute loss.*

Wishing next to determine whether, by lengthening the interval between the division of the two nerves, a few days more, the life of the animal could not be protracted to a greater length, or even saved, I made another experiment.

EXPERIMENT.

Having divided one of the nerves of the eighth pair, and waited the lapse of nine days, I divided the other. The same symptoms came on now as in the last experiment, but scarcely so violent. The only kind of food he would take was milk, and that in small quantities, and this always produced great uneasiness at the stomach, with symptoms of indigestion. In this state he continued thirteen days, and then died, very much emaciated.

From this dog having lingered so long, I was beginning to

entertain hopes of his recovery, and had that eventually happened, I doubt much whether, even under the present uncertainty of things, I could have resisted the temptation of ascribing such recovery to the reproduction of the nerves; but the event put a stop to my speculation.

I think I have now proved my first position, (viz.) that whether the eighth pair of nerves be divided in immediate succession, so as to deprive an animal of their influence suddenly, or whether this deprivation be effected in a more gradual way, the consequences are in the end equally fatal. I must next endeavour to avail myself of this fact in the solution of the problem now before me. If the substance of nerve be reproduced, certainly a period longer than the above must be necessary for this process; but to mark the precise point of time when the line is to be drawn, would require the sacrifice of more animals than a question of mere curiosity could justify. I must, therefore, content myself with giving a general answer to the question, and inquire whether, by suspending the division of the second nerve for a much greater length of time than was done in the two last experiments, the existence of the animal could be preserved.

EXPERIMENT.

Another dog being procured, and one of the nerves of the eighth pair divided, I allowed six weeks to elapse before the other was cut through. This division of the corresponding nerve evidently deranged him; but in a much less degree than in the former experiments. For some days he refused solid food, but took milk; afterwards he ate solid food in small quantities; and near a month had passed away before he fed

as usual. The actions of the stomach were for a long time evidently deranged, so that he was continually harassed with symptoms of indigestion; and six months had nearly elapsed before he recovered his health, though during five months of the time he took his usual quantity of food.

Now, to what cause are we to impute his recovery? The most probable one appears to be, that in the interval of six weeks the first nerve had been reproduced; so that the actions of those organs depending upon this nerve, though somewhat disturbed, were not suspended. But as the union of the second nerve advanced, and the reproduction of the first became more perfect, the vital organs gradually recovered their healthy state.

I kept this animal nineteen months, during the greatest part of which time he performed the office of a yard dog. And here it may be proper to observe, that in all the experiments, the voice was totally lost on the division of the second nerve. This effect anatomists will easily understand, from recollecting that the recurrent branches of the eighth pair, which are the true vocal nerves, originate below the part where the trunks of the eighth pair were cut through; consequently those nerves are themselves in effect divided. Now it deserves to be remarked, that his voice returned in proportion as his general health improved; and in about six months he could bark as strongly as before, but the pitch of his voice was evidently raised.

From this experiment, I am strongly inclined to believe that there must have been a true reproduction of the nerve; yet I do not contend, that if the part of union were examined by an anatomical eye, such reproduction would be very evi-

dent. On the contrary, I am persuaded that anatomy can determine only the presence and existence of an uniting medium; but it is the province of physiology to decide whether the medium of union possess the characters, and perform the function, of the original nerve.

The evidence of reproduction, as resting on this experiment, may not be sufficient to obviate certain doubts, which reflections upon this subject may probably suggest. There is a difficulty which naturally presents itself here, and this is, the possibility of the stomach and vocal organs having received an additional supply of nervous energy from another source. And to give an appearance of validity to this objection, it may be said that the eighth pair of nerves communicates energy to the larynx by means of the laryngeal branch, and that this branch arises from the trunk above the part where the division was made, and consequently its function received no interruption from the experiment. Again, with regard to the stomach, another apparent objection offers. This organ receives nerves from the great sympathetic, as well as the eighth pair; and nothing hitherto advanced has tended to disprove, that the defect of nervous influence from the division of the latter, has been supplied by greater exertions of the former. Lastly, the familiar analogy of the vascular system, where collateral branches are enlarged from the obliteration of a principal trunk, tends further to give weight to these doubts.

To remove these seeming difficulties by anatomical investigation, or by directing my views to any changes that might be induced on the anastomosing nervous filaments, would be an undertaking not less tedious in its execution than unsatisfac-

tory in its result; for there would still remain room for opposite opinions: and while some would argue that these anastomosing filaments were become evidently enlarged, others would contend that they had not suffered the slightest change.

Now, I have already expressed my distrust of those decisions which are founded on an appeal to the eye, seeing that anatomy has yet to explain by what mechanism or structure these organs perform their office; and because I have frequently heard opposite opinions on my own preparations. I therefore prefer an appeal to the functions of these parts, and inquire whether, in the experiment in which the dog survived the division of the second nerve of the eighth pair after an interval of six weeks, it was effected by the reproduction of the first divided nerve, or in another way?

There are only two possible answers to such a question; these are, that either the functions of the stomach, larynx, &c. were carried on by anastomosing nerves; or that the united nerves had recovered their original importance.

If the first be contended for, this consequence ought to ensue, (viz.) that the eighth pair should now be entirely useless, and both of them may be divided a second time, without injuring any of the functions of the animal.

If the last be granted, it must of necessity follow, that the medium of union possessed the same properties as the original nerve.

I have now circumscribed the field of inquiry, and have drawn the question into so narrow a compass, that it is in the power of a single experiment to prove either the affirmative or negative. If now the eighth pair be divided a second time in immediate succession, and the animal sustain it with impu-

nity, I conceive it right to conclude, that the actions of those organs, which originally were carried on through the means of the eighth pair, are now performed by other channels, and that the true substance of the nerve is not reproduced. But on the contrary, if the animal die in consequence of it, then I think it equally just to infer, that the new formed substance is really and truly *nerve,* because we know of no other substance which can perform the office of nerve.

I shall rely then upon the following, and consider it as my *experimentum crucis.*

EXPERIMENT.

Having the dog in my possession upon which I divided the eighth pair of nerves nineteen months before, I cut through both of them now, in immediate succession. The usual symptoms were immediately induced, and continued until the second day, when he died.

After death I carefully dissected out these nerves, and have preserved them as evidences of my success. I think I have now answered the question I proposed to myself, and can affirm that nerves are not only capable of being united when divided, but that *the new formed substance is really and truly nerve.*

I forbear to make any animadversions on the experiments of those who have formed conclusions contrary to my own: to such I can only say, that I shall always consider myself highly honoured in having the opportunity of showing them the result of my own experiments; and, as far as these will allow me, *to convince by ocular demonstration, though I should fail to persuade by argument.*

FIG. 1.

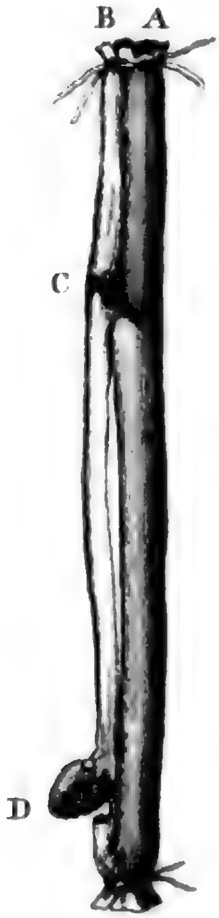
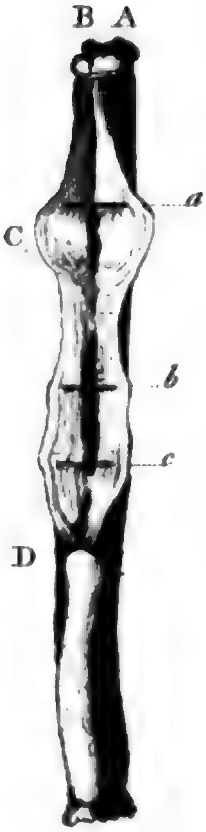


FIG. 2.

FIG. 3.



D. Blane delin.

J. Caldwall sc.

EXPLANATION OF THE PLATE (Tab. XVII.)

The three figures are taken from preparations now in the author's possession, being the result of some of the experiments related in the paper.

In each figure the nerve is represented in connection with the carotid artery, to which it naturally adheres by cellular membrane.

Fig. 1st. A, the carotid artery.

B, one of the nerves of the eighth pair.

C, the part where the first division was made, as it appeared after nineteen months.

D, the part where the second division was made, and from which the dog died on the second day.

Fig. 2d. A and B, the carotid artery and nerve of the opposite side.

C, the union which followed the first division, forming a swell like a ganglion.

D, the second division, made two days before death.

Fig. 3d. The same nerve cut open.

a, *b*, *c*, represent bristles to keep the cut surfaces asunder.

VIII. *The* Croonian *Lecture on Muscular Motion.* *By* Everard Home, *Esq. F.R.S.*

Read November 11, 1790.

WHEN I recollect the many learned men who have given this lecture, I cannot but feel myself much flattered by the honour of being named to that office; I feel, at the same time, my own inability to explain many of the phænomena of muscular motion; yet more its principle, the subject to which this lecture was originally confined.

The many, and perhaps insuperable, difficulties which obstruct our progress towards that knowledge, have led the ablest anatomists and physiologists, who have been called upon by this learned Society for their observations upon muscular motion, to deviate from the original intention of the founder, and instead of attempting an investigation of the principle, to explain the anatomical structure, and various phænomena of muscles with which they were acquainted; that by this means they might furnish data for future inquiries.

I shall consider the example of such men as sufficient authority for not confining myself too closely to the subject prescribed; and content myself with giving such facts and observations respecting muscles, as have not, I believe, been already laid before this learned Society.

This lecture was given for several years by Mr. HUNTER,

who still continues to prosecute the subject; and should the following observations contain any new materials, it is from that source that many of them are derived: for in my peculiar situation, I should little merit the honourable task assigned to me, were I not to avail myself of every advantage in my power, that could make the present lecture worthy the attention of this learned audience.

The principle of action in an animal, appears to be as extensive as life itself, and is almost the only criterion by which we can distinguish living matter from dead.

This action does not seem to depend so much upon structure, as upon a property connected with life, which is equally extensive in its principle, and so far as we are yet acquainted, equally concealed from the researches of human sagacity.

To acquire a sufficiently enlarged notion of this principle, we must not confine our inquiries to one set of animals, but must take into our view the whole chain of animated beings; and from a review of the different circumstances in which it occurs, and the varied structure of parts upon which it is impressed, we shall have sufficient evidence that the fasciculated fibrous structure commonly met with is not necessary to its existence, but only made use of for its support, and continuance.

The structure which produces muscular action, varies so much in different animals, that we are at a loss to conceive how the effects should have the least similarity; and it is in some cases, only from witnessing the actions that we can consider the parts as muscles; since in nothing else do they bear a resemblance to the muscular structure in the more perfect animals with which we are best acquainted.

We shall illustrate this observation by a description of the

structure, and actions, of the animals called hydatids, which appear from their simplicity to be the furthest removed from the human; for as the human is the most complicated, and most perfect in the creation, the hydatid is one of the most simple, and composed of the fewest parts. It is to appearance a membranous bag, the coats of which are so thin as to be semitransparent, and to have no visible muscular structure. From the effects produced by the different parts of this bag while the animal is alive, being exactly similar to the contractions and relaxations of the muscular fibres in the human body, we must conclude that this membrane is possessed of a similar power; and consequently, has the same right to be called muscular.

The hydatid, from its apparent want of muscles, and other parts which generally constitute an animal, was for a long while denied its place in the animal world, and considered as the production of disease; we are, however, at present in possession of a sufficient number of facts, to ascertain, not only that it is an animal, but that it belongs to a genus of which there are several different species.

Hydatids are found to exist in the bodies of many quadrupeds, and often in the human; the particular parts most favourable to their support appear to be the liver, kidneys, and brain, although they are sometimes detected in other situations.

One species is globular in its form, the outer surface of the bag smooth, uniform, and without any external opening; they are seldom found single, and are contained in a cyst, or thick membranous covering, in which they appear to lie quite loose; having no visible attachment to any part of it. This species is most frequently found in the liver and kidneys, both of the

quadruped and human subject. They vary in size, but those most commonly met with, are from one quarter of an inch to three quarters of an inch in diameter.

Another species is of an oval form, with a long process, or neck, continued from the smallest end of the oval, at the termination of which, by the assistance of magnifying glasses, is to be seen a kind of mouth; but whether this is intended merely for the purpose of attachment, or to receive nourishment, is not easily determined. This species is found very commonly in the brain of sheep, and brings on a disease called by farmers the staggers. It is not peculiar to any one part of the brain, but is found in very different situations, sometimes in the anterior, at others in the posterior lobe. It is inclosed in a membranous cyst like the globular kind; but differs from that species in one only being contained in the same cyst; and the bag, or body of the animal, being less turgid, appearing to be about half filled with a fluid, in which is a small quantity of white sediment; while the globular ones are in general quite full and turgid.*

This species, from its containing only a small quantity of fluid, has a more extensive power of action on the bag, and is therefore best fitted for illustrating the muscular power of these animals.

If the hydatid be carefully removed from the brain, immediately after the sheep is killed, and put into warm water, it will soon begin to act with the different parts of the body, exhibiting alternate contractions, and relaxations. These it performs to a considerable extent, producing a brisk undulation

* The species of hydatid without a neck is also met with in the brains of sheep, but is less turgid, and less of a spherical figure, than those commonly found in the liver.

of the fluid contained in it; the action is often continued for above half an hour, before the animal dies; and is exactly similar to the action of muscles in the more perfect animals. This species of hydatid, is very well known by the name *tænia hydatigenia*; it varies considerably in its size; one of those which I examined alive, was above five inches long, and nearly three inches broad at the broadest part, which makes it nine inches in the circumference.

The coats of the hydatid, in their recent state, exhibit no appearance of fibres, even when viewed in the microscope; but when dried, and examined by glasses of a high magnifying power, they resemble paper made upon a wire frame. This very minute structure is not met with in membranes in general; it may therefore be considered as the organization upon which their extensive motions depend.

The coats of the different species of hydatids had all of them the same appearance in the microscope.

The intestines, in some of the more delicately constructed animals, have a membranous appearance, similar to the bag of the hydatid, and we cannot doubt of their possessing a muscular power, since there is no other mode of accounting for the food being carried along the canal. The action of the intestines, not coming so immediately under our observation, makes them a less obvious illustration of this principle than the hydatid; we may, however, consider their having a similar structure, as a strong confirmation of it.

If we compare the structure of muscles in the human body, with that of the membranous bag, which composes the *tænia hydatigenia*, a structure evidently endowed with a similar principle of action, the theories of muscular motion, which are

founded upon the anatomical structure of a complex muscle, must be overturned.

The simplicity of form, in the muscular structure of this species of hydatid, makes it evident, that the complex organization of other muscles, is not essential to their contraction and relaxation, but superadded for other purposes; which naturally leads us to suppose, that this power of action, in living animal matter, is more simple, and more extensively diffused through the different parts of the body, than has been in general imagined.

From these observations we shall find, that the inquiries hitherto made, into the principle of muscular motion, by investigating the muscles of the more perfect animals, which are most remarkable in their effects, and obviously most deserving of attention, have been too confined.

From our inquiry into the structure of muscles, in different animals, we readily discover, that those above mentioned, although the most perfect in their organization, are at the same time so complicated, for the purpose of adapting them to a variety of secondary uses, that they become of all others, the kind of muscle least fitted for the investigation of the principle itself.

In the present imperfect state of our knowledge respecting animal life and motion, a physiologist, who would select a complex muscle, with the view of discovering, from an examination of its structure, the cause of muscular contraction, would resemble a man, ignorant of mechanics, who should consider a watch as the machine best constructed to assist his inquiries respecting the elastic principle of a spring; which, at first sight, must appear absurd. For although the spring is the power

by which the motions are all produced, the machine is so complicated with other important or necessary parts, that the spring itself is not within the reach of accurate observation.

To prosecute an inquiry into the cause of muscular motion, with the greatest probability of success, recourse should be had to muscles, which are in themselves the most simple; and we should endeavour to ascertain what organization, or mechanism, is essential to this action in living animal matter, by which means we should acquire a previous step to the investigation of the principle itself.

The complex muscles in the more perfect animals, from their structure and application, open a wide field of inquiry; for we shall find that it is from their different organizations, that they are enabled to perform the various actions of the body; actions too powerful and extensive for muscles to effect, unaided by such complication of structure, and the advantages derived from it.

In the present lecture, I shall confine myself to the consideration of the most important uses of the complex structure of muscles, and by this means make it evident, that they are not indebted to it for the principle upon which muscular motion depends.

These complications are necessary to supply the muscle with nourishment, for the continuance of its action; to give it strength; to enable it to vary its contraction from the standard or ordinary quantity; and to increase the effect beyond the absolute contraction of the muscle. How these different purposes are effected, I shall endeavour to explain.

A muscle receives its nourishment from the blood, with which we find it more abundantly supplied than most other

parts of the body. This supply is evidently intended for the support of its action, since it is proportioned to the exertions of the muscle; and whenever a muscle is rendered incapable of acting, which frequently happens from the joints becoming stiff, the quantity of blood sent to it is very much diminished. The great vascularity of a muscle is, therefore, for the purpose of repairing the waste in the muscular fibres, occasioned by their action; and without this support, the continuance of their contractions would be of short duration.

The strength of a muscle must depend upon the number of its fibres, and most probably upon their size; since in strong muscles the fibrous appearance is very obvious, while in very weak ones no such structure is visible to the eye. A distinction of fibres has been considered as essential to the contraction of a muscle, and only those parts have been allowed to possess that power, in which fasciculi of fibres could be ascertained. But from the observations which have been made, it would perhaps be nearer the truth, to consider the circumstance of the fibres being distinct, as a proof of strength in a muscle, but not essential to the existence of muscular contraction.

There is a power inherent in a complex muscle, by which it can increase or diminish the ordinary extent of its contraction; this is very curious, and must arise from some change going on in the muscle itself, for which it is adapted by means of this very complicated organization.

The usual quantity of contraction which takes place in the fibres of a complex muscle, in the different motions of the human body, is adapted in the nicest manner to the circumstances in which the muscle is placed; and the quantity of contraction appears to be limited by the fibres having no power

of becoming shorter. We find, however, from observation, that when the extent of motion in a joint, or the distance between the fixed points of the muscle, is accidentally altered, the muscle acquires a power of adapting its quantity of contraction to the new circumstances which have taken place.

This power in a muscle may be considered as a proof that the principle of contraction is independent of its particular organization; since it can undergo a complete change within itself, so that its fibres shall be shortened to one half of their original length, and still have the same contractile power as when in its original state.

The extent of this principle is well illustrated by the following case. A negro about thirty years of age, having had his arm broken above the elbow joint, the two portions of the os humeri were unfortunately not reduced into their places, but remained in the state they were left by the accident, till the callus or bony union had taken place; so that when the man recovered, the injured bone, from the position of the fractured parts, was reduced almost one half of its length. By this circumstance, the biceps flexor cubiti muscle, which bends the fore-arm, remained so much longer than the distance between its origin and insertion, that in the most contracted state it could scarcely bring itself into a straight line: this muscle, however, in time, as the arm recovered strength, adapted itself to the change of circumstances, by becoming as much shorter as the bone was diminished in length; and by acquiring a new contraction in this shortened state, it was enabled to bend the fore-arm.

Some years after this accident, the person died, and the circumstances abovementioned being known, the parts were exa-

mined with particular attention. The biceps muscles of both arms were carefully dissected out, and being measured, the one was found to be eleven inches long, the other only five; so that the muscle of the fractured arm had lost six inches, which is more than the half of its original length. These muscles are now deposited in Mr. HUNTER's collection of preparations illustrating the animal œconomy.

That muscles possessed this power, has been taken notice of by Mr. HUNTER in a former lecture; but the instance which I have given, is so striking an illustration of this principle, that I could not avoid mentioning it while upon this subject.

Muscular contraction is an operation, in whatever way performed, by which the vital stores of the animal are considerably exhausted; this is evident from the quantity of blood with which muscles whose action is frequent are supplied.

This expence would appear, from observation, to be occasioned rather by the extent of contraction, than by its frequency, or force; for if we examine the mechanism of an animal body, we shall find a variety of structures evidently intended for no other purpose than diminishing, as much as possible, the necessary extent of contraction in muscular fibres, while there is no such prevention of frequency of action.

Muscles in general are applied to the bones in such way as to act with great mechanical disadvantages as to power; but this is more than compensated by the small quantity of contraction which is required; and in the muscles of respiration, we find frequency of action is preferred to an increased quantity of muscular contraction.

The velocity of motion thus acquired, although a considerable advantage, does not seem to have been the principal

object intended by such structure, but rather to procure the effect by means of short contractions, which are less fatiguing, or in some other way more in the management of the constitution, than long ones.

That long contractions in a muscle cannot be supported for any length of time, may be illustrated from the actions both of the voluntary and involuntary muscles.

While the voluntary muscles are under the command of the will, we cannot ascertain what would be the effects produced by the continuance of their contractions, since the influence of the brain communicated by the nerves becomes soon weakened, and puts a stop to their action; but when the contractions of voluntary muscles are by any circumstance rendered involuntary, the difference in the time of their continuance appears to be in the inverse proportion of the quantity of contraction; for muscles, whose usual functions consist in short contractions, can go on for a long time, while those which are performed by long contractions soon cease.

In the muscles of a paralytic arm, their action, to a certain extent, is continued for years (the times of sleeping excepted), without any effect being produced upon the constitution, or the parts themselves; but in epileptic fits, in which the actions are equally involuntary, only requiring longer contractions, they soon cease, leaving the person greatly exhausted; an effect which must arise from the quantity, not the frequency, of the contractions.

If we attend to the actions of the involuntary muscles, we find that they are continued through life, but that the quantity of contraction is very small; and if from any circumstance the quantity should be increased, it cannot be continued, the

parts being unable to sustain it for any length of time. The diaphragm, and intercostal muscles, act constantly in performing the functions of respiration, but they do not exert themselves to their full extent. In laughing, which is likewise an involuntary action, the contractions of these muscles are more extensive, therefore if continued beyond a very short period become so distressing, that a cessation necessarily ensues.

Muscular contraction is never made use of in an animal body, where any other means can produce the same effect, and for this reason elastic ligaments are frequently substituted for muscles; even where muscles are employed, various means are applied to diminish the quantity of contraction.

It is curious, in tracing the different forms of muscles, and in considering the uses for which they are employed, to observe how variously the fibres are disposed, evidently for the purpose of obviating the necessity of great contractions; and the quantity of muscular action saved by this mechanism is greater, in proportion to the frequency and importance of the effect the muscle is intended to produce: this appears to be invariably the case.

Muscles only occasionally called into action, have their fibres nearly straight, which gives no mechanical advantage; the sartorius is an instance of this kind.

Muscles frequently used are more complicated, as those of the fingers are half penniform in their structure; the muscle for raising the heel in walking is penniform; that which raises the shoulder, complex penniform; and those of the ribs, cruciform.

That the two sets of intercostal muscles act at the same time, I proved by experiment in the year 1776. I removed

a portion of the external intercostal muscles from the chest of a dog, and in that way saw very distinctly the two sets of muscles in action. The fibres of both sets contracted exactly at the same time.

The particular structures of these different forms of muscles, and the mechanical advantages arising out of them, have been already explained in former lectures upon this subject; but there is a form of muscle, in which the disposition of fibres produces a considerable saving of muscular contraction, that has not been at all taken notice of.

The muscle I allude to is the heart, the most important in the body, whether we consider the frequency of action, or the office in which that action is employed; and we shall find, upon examination, that the fibres are disposed differently from those of any other muscle, which disposition of fibres appears to have a superiority, in being enabled to produce their effect by a smaller quantity of contraction.

In considering the muscular structure of the heart, it is only intended to examine that part of it called the ventricles, which may be reckoned two separate muscles. The right ventricle, for sending the blood through the vessels of the lungs, called the lesser circulation; the left, to propel it through the branches of the aorta, which go to every part of the body, called the greater circulation.

If these two ventricles are superficially examined, the muscular partition by which they are united seems to belong equally to both, one half of it appearing to be a portion of the right, the other of the left ventricle.

In this view, the sides of the left ventricle, although evidently more muscular and thicker than those of the right, are

by no means stronger in proportion to the difference of effects they have to produce. We find, however, upon dissection, that the septum is almost wholly a portion of the left ventricle, which gives it a great superiority over the other, and makes it capable of performing the important office of supplying the body with blood.

The left ventricle of the heart, detached from the other parts, is an oviform hollow muscle, but more pointed at its apex than the small end of a common egg. It is made up of two distinct sets of fibres, laid upon one another in the form of strata; those which compose the outer set have their origin round the root of the aorta, and in a spiral manner surrounding the ventricle to its apex, or point, where they terminate, after having made a close half turn. The fibres of the inner set, or stratum, are similar to those of the outer, in their origin, in the mode of surrounding the cavity, and in their termination, but their direction is exactly the reverse; they decussate the outer set in their whole course, and where the two sets terminate, they are both blended into one mass. There is an advantage gained by this disposition of fibres over every other in the body, which adapts the ventricle so perfectly to its office, that it would almost appear impossible to construct it in any other way, so as to answer the purposes for which it is intended.

In this muscle, the fibres, by their spiral direction, are nearly one fourth part longer than the distance between the origin and insertion; and the action of the two sets being in different directions, renders only one half the quantity of contraction in each fibre necessary, that would have been otherwise required; while the turn both sets make in opposite directions

at the apex of the ventricle, fixes it and prevents lateral motion.

In the action of the ventricle, two different effects are produced; the first brings the apex nearer to the basis, by which means the *vis inertiæ* of the blood will be overcome where the resistance is least, and a direction given to its motion in the course of the aorta; the second brings the sides nearer each other, which will accelerate the motion of the blood already begun; and the spiral direction of the fibres, will render the power which is applied, more uniform through the whole of that action, than it could have been made by any other known form of muscle; the spiral action will also readily shut the valvulæ mitrales, while the apex is drawn up, which could only be effected by this particular construction.

By this beautiful mechanism, which I have endeavoured to describe, the muscular fibres of the left ventricle of the heart perform their office with a smaller quantity of contraction, compared to their length (although in themselves proportionally longer), than those of any other muscle in the body, and consequently produce a greater effect in a shorter time.

The right ventricle is situated upon the outside of the left, with which it is firmly united; it is not oviform in its shape, but triangular; nor is it uniform in its structure, being made up of two portions, whose fibres have a very different distribution.

The portion of this ventricle which makes a part of the septum of the heart, consists of only one set of fibres, similar in their direction to those of the stratum underneath, belonging to the left ventricle; but from being considerably shorter, they are more oblique than the spiral; and at the edge of the

cavity they are blended with the fibres of the opposite portion.

That portion which is opposite to the septum is composed of three sets of fibres; those of the external set are nearly longitudinal; the two others, which lie under it, decussate each other, and are obliquely transverse in their direction, one passing a little upwards, the other downwards; and both terminate upon the edge of the septum.

In the structure of this muscle we find none of the mechanical advantages, so obvious in the left ventricle; the want of these, however, is in some measure compensated by its situation; for the blood contained in its cavity, will have the *vis inertiæ* overcome, and a direction given to its course by the action of the apex of the left ventricle: that motion only requiring to be continued, and accelerated, for which purpose the structure of this muscle is very well calculated; and in which it will also be assisted by the lateral swell of the septum into its cavity, in the contraction of the left ventricle.

In the course of this lecture, it has been my endeavour to show the most simple structure that is capable of muscular action; and to point out the advantages intended to be produced by the different complications which occur in an animal body.

The view which I have taken of this subject gives us an idea of the extent to which muscular action is employed in different animals; and leads to the belief, that very dissimilar structures in the more perfect animals are endowed with this principle, since the actions of the smaller arteries, as well as of the absorbent vessels, must be referred to it.

To ascertain whether any such action could be demonstrated in the membranes of the quadruped, I made the following experiments.

These experiments were made upon the internal membrane of the urinary bladder of a dog, which, in consequence of the animal dying a violent death, was in a very contracted state; the whole of its contents having been expelled in the act of dying.

The method I have adopted to ascertain the muscular power of this membrane, is similar to that taken by Mr. Hunter in his very ingenious investigation of the structure of blood-vessels, which was laid before this Society; the same mode being equally applicable to the present subject.*

The bladder was carefully laid open, and a portion of its internal membrane, which was corrugated into folds, was dissected off. This portion was spread out, so as to be completely unfolded; it was then laid upon a piece of plate glass wetted, to prevent, as much as possible, any friction; its exact length, in this contracted state, was three quarters of an inch; it was now stretched out, and found to be $1\frac{3}{8}$ inch, upon being left to itself, it contracted so as to be only 1 inch, so that in this state it had gained $\frac{2}{8}$ of an inch, which must have been lost by some action in the living body, and entirely independent of its elasticity. This portion of membrane then had two powers of contraction, one which was muscular, and equal to $\frac{2}{8}$ of an inch, the other elastic, and equal to $\frac{3}{8}$ of an inch.

Another portion of the same membrane, $\frac{1}{2}$ an inch long and

* Mr. Hunter's experiments on the arteries of the horse are published in his treatise on the Blood, Inflammation, and Gun-shot Wounds.

$\frac{5}{8}$ broad, was treated in the same way, and its muscular contraction was found to be $\frac{1}{8}$ of an inch, that from elasticity $\frac{4}{8}$ of an inch.

A third portion of membrane $\frac{7}{8}$ of an inch long, and $\frac{2}{8}$ broad, was ascertained to have contracted $\frac{2}{8}$ of an inch by its muscular power, and $\frac{3}{8}$ from its elasticity.

It will scarcely be necessary to mention, that the muscular contraction in this membranous structure, is very readily overcome, since this must be almost self-evident; that circumstance, however, must be particularly attended to in making similar experiments.

The internal membrane of the urethra we know to be capable of contracting, as spasmodic strictures are formed in that canal. This membrane, when dried and examined in the microscope, has not the same appearance as the coats of the hydatid; but the whole is a congeries of vessels forming a network. We must, therefore, suppose that the action is in these very minute vessels.

From these experiments and observations, membranous structures are found to exert an action hitherto denied them; and it is equally evident, that this principle is applied to the purposes of the animal œconomy in a more extensive manner than has been generally imagined.

To explain even the most obvious phænomena of muscular motion, must appear from the above observations to be attended with difficulty; how arduous then the task of investigating the principle upon which that motion depends; a principle as extensive as life itself, with which it is coeval, and indeed the only criterion we have of its existence.

An endeavour to throw light upon that principle, has not been the object of the present lecture; I have only attempted to state some circumstances respecting the mechanism employed in producing muscular motion, leaving to others the prosecution of this most intricate and difficult inquiry.

ERRATA.

Page 42, line last, *for* C, *read* c.
Page 115, line last but one, *instead of* B. ad H. *perhaps it should have been* B. ad C.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 1	29	8	0	30	49	29,82	73		NE	1	Fair.
	36	2	0	36	51,5	29,87	64		ENE	1	Fine.
2	29	8	0	29,5	48,5	30,00	71		ENE	1	Fine.
	35	2	0	35	50	30,10	71		ENE	1	Cloudy.
3	30	8	0	30	47,5	30,35	70		NE	1	Fine.
	36	2	0	36	51	30,41	69		E	1	Cloudy.
4	29	8	0	29	47	30,44	70		SE	1	Cloudy.
	32	2	0	32	49	30,39	70		SE	1	Cloudy.
5	32	8	0	32	47	30,24	69		E	1	Cloudy.
	34	2	0	34	48	30,20	69		E	1	Cloudy.
6	31,5	8	0	32	46	30,12	70		E	1	Cloudy.
	34	2	0	34	49	30,07	65		E	1	Cloudy.
7	31	8	0	31	46	30,18	64		NE	1	Cloudy.
	37	2	0	37	49	30,24	65		NE	1	Cloudy.
8	30	8	0	33,5	46,5	30,40	73		ESE	1	Cloudy.
	35	2	0	35	51	30,39	74		ESE	1	Fair.
9	25	8	0	26	46	30,37	80		SW	1	Foggy.
	28	2	0	27,5	48	30,34	79		SSE	1	Foggy.
10	22	8	0	22,5	44	30,30	76		SSE	1	Cloudy.
	30,5	2	0	30,5	48	30,27	77		SSE	1	Foggy.
11	23,5	8	0	32	43	30,07	84		N	1	Cloudy.
	37	2	0	36	46,5	29,90	84		E	1	Cloudy.
12	31	8	0	32	43,5	29,78	82		SSE	1	Cloudy.
	34	2	0	34	47	29,81	75		S	1	Cloudy.
13	30,5	8	0	31	43,5	29,98	78		WSW	1	Cloudy.
	33,5	2	0	33,5	48	30,01	82		NW	1	Cloudy.
14	30,5	8	0	32,5	44	30,05	84		W	1	Fair.
	43,5	2	0	43,5	49	30,05	78		SW	1	Cloudy.
15	37,5	8	0	37,5	46	30,07	73		SSW	1	Cloudy.
	41,5	2	0	41,5	51	30,10	71		SSW	1	Cloudy.
16	31,5	8	0	31,5	47	30,30	79		W	1	Fair.
	40,5	2	0	40,5	50	30,35	74		NE	1	Fine.

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1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Jan. 17	32	8	0	36,5	47	30,33	79		W	1	Cloudy.
	43	2	0	42,5	51	30,38	76		NW	1	Fair.
18	32	8	0	32,5	47	30,42	79		SW	1	Cloudy.
	43	2	0	43	51	30,40	83		SW	1	Cloudy.
19	37	8	0	37	49	30,51	86		W	1	Foggy.
	40	2	0	40	51	30,55	85		SW	1	Cloudy.
20	40	8	0	40	49	30,53	80		WSW	1	Cloudy.
	46	2	0	46	54	30,54	71		WNW	1	Fine.
21	35,5	8	0	38	50	30,54	83		W	1	Foggy.
	47	2	0	47	53	30,56	77		W	1	Cloudy.
22	42,5	8	0	43	51,5	30,53	83	0,048	W	1	Foggy.
	45	2	0	45	53	30,45	67		WSW	1	Cloudy.
23	41	8	0	43	51	30,03	70		SW	2	Cloudy.
	47	2	0	47	53	29,91	72		SW	2	Cloudy.
24	35	8	0	35	51	29,88	69		WSW	2	Fair.
	42	2	0	42	52	29,72	66		SW	2	Hazy.
25	36	8	0	36	50	29,71	70	0,105	SW	2	Fair.
	40	2	0	34	53	29,55	67		WNW	2	Sleet.
26	26,5	8	0	28	48	29,22	68		WNW	1	Cloudy.
	32	2	0	32	50	29,32	58		WNW	1	Fine.
27	26,5	8	0	31	47	28,75	86		E	1	Snow.
	31	2	0	31	48	29,10	75		NW	2	Hazy.
28	25,5	8	0	30	46	29,28	76		NW	1	Cloudy.
	34	2	0	34	50	29,46	70		NW	1	Fine.
29	31	8	0	32	46,5	29,52	78		WNW	1	Cloudy.
	37	2	0	36	50	29,58	74		W	1	Hazy.
30	31,5	8	0	32	46	29,68	76		WNW	1	Fair.
	48	2	0	39	49,5	29,82	70		WSW	1	Hazy.
31	48	8	0	48	48	29,76	88	0,250	SW	2	Cloudy.
	51	2	0	50	52	29,78	80		S	2	Cloudy.

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for February, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Feb. 1	46	7	0	46	51	29,68	79		SSW	2	Fair.
	50	2	0	49	56	29,61	72		SSW	2	Fine.
2	44	7	0	44	52	29,72	75		SSE	2	Fair.
	49,5	2	0	49,5	55	29,75	65		SSE	2	Cloudy.
3	42	7	0	42	53	29,90	73		S	2	Cloudy.
	49,5	2	0	48	55	30,02	67		SWb.S	1	Fair.
4	35	7	0	36	53	30,14	74		S	1	Fine.
	44	2	0	44	56,5	30,18	67		S b. W	1	Fine.
5	38	7	0	38	53	30,17	74		ESE	1	Cloudy.
	45	2	0	45	56	30,14	67		ESE	1	Cloudy.
6	40	7	0	40	53	29,98	72		E	1	Cloudy.
	43	2	0	43	55	29,93	73		ESE	1	Cloudy.
7	38	7	0	40	53	30,05	78		SSW	1	Cloudy.
	43	2	0	44	55	30,05	78		S	1	Rain.
8	41,5	7	0	42	53	30,18	75	0,076	SW	1	Cloudy.
	49	2	0	49	57	30,27	70		WSW	1	Cloudy.
9	44	7	0	44	54	30,25	78		SW	1	Cloudy.
	51	2	0	51	58	30,24	70		WSW	1	Fair.
10	44	7	0	44	55	30,13	74		WSW	1	Cloudy.
	48	2	0	48	57,5	30,14	58		W	2	Fair.
11	38	7	0	40	54	30,05	71		SW	1	Cloudy.
	47	2	0	47	57	29,83	79		SSW	1	Rain.
12	48	7	0	51	56	29,66	69	0,068	WSW	2	Cloudy.
	51	2	0	50	59	29,81	71		WNW	2	Cloudy.
13	46	7	0	46	56	29,85	75		WSW	1	Cloudy.
	50	2	0	49	58	29,81	68		SW	1	Cloudy.
14	50	7	0	50	58	29,66	73		WSW	1	Cloudy.
	56	2	0	56	60	29,69	70		W	2	Cloudy.
15	50	7	0	51	58	29,52	76		W	2	Cloudy.
	55	2	0	54	62	29,57	59		W	2	Fair.
16	44	7	0	46	58	29,65	75	0,294	E	1	Cloudy.
	51,5	2	0	51	58	29,62	75		SW	1	Cloudy.

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1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Feb. 17	47	7	0	47	58	29,70	80	0,015	SSW	1	Rain.
	49	2	0	48	59	29,69	70		SSW	2	Cloudy.
18	42	7	0	43	57	29,84	72		NE	1	Cloudy.
	44	2	0	44	58	29,94	70		ENE	1	Cloudy.
19	39	7	0	39	56	30,01	70		E	1	Cloudy.
	46,5	2	0	46,5	57	29,99	67		E	1	Cloudy.
20	44	7	0	46	56	29,77	74		S	1	Cloudy.
	52	2	0	52	58	29,66	77		SSW	2	Cloudy.
21	50	7	0	52	57	29,58	78	0,037	SSW	2	Cloudy.
	55	2	0	55	59,5	29,51	75		SSW	2	Cloudy.
22	48	7	0	51	57,5	29,78	79		SSW	2	Cloudy.
	53	2	0	53	60	29,65	75		SSW	2	Cloudy.
23	45	7	0	45	58,5	29,40	74		SSW	2	Fine.
	55	2	0	55	60	29,47	60		SSW	2	Fair.
24	47	7	0	48	58	29,51	75	0,048	SSW	2	Cloudy.
	56	2	0	53	60	29,51	75		SSW	2	Cloudy.
25	49	7	0	51	59	29,68	77	0,062	SW	1	Cloudy.
	55	2	0	54	61	29,65	74		SSW	2	Cloudy.
26	40	7	0	40	57	30,00	68	0,032	W	1	Fair.
	48	2	0	47,5	59,5	30,11	58		WNW	2	Fair.
27	38,5	7	0	40	57	30,29	70		WNW	1	Cloudy.
	49	2	0	49	59	30,24	62		WNW	1	Hazy.
28	43,5	7	0	44	56	29,87	76	0,023	SSW	2	Rain.
	51	2	0	51	59	29,75	64		WNW	2	Fair.

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for March, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar. 1	35	7	0	36	55	29,94	72	0,032	WNW	1	Hazy.
	46,5	2	0	46	58	29,83	67		SW	1	Cloudy.
2	38	7	0	40	56	29,63	74	0,088	WNW	1	Rain.
	47	2	0	47	57	29,73	66		WNW	1	Cloudy.
3	38	7	0	44	55	29,85	73	0,022	S	2	Cloudy.
	49	2	0	49	56,5	29,80	75		S	2	Cloudy.
4	49	7	0	49	56	29,90	56	0,045	S	2	Cloudy.
	54	2	0	53	59	29,96	70		S	2	Fair.
5	47	7	0	48	57	29,90	67		S	1	Hazy.
	50	2	0	50	58,5	30,02	56		S	1	Fair.
6	37	7	0	39	54,5	30,18	66		SW	1	Fair.
	50	2	0	50	58	30,13	65		S	1	Cloudy.
7	41,5	7	0	42	56	30,11	74		S	1	Fine.
	54	2	0	54	60	30,11	57		S	1	Fine.
8	38	7	0	39	57	30,14	70		S	1	Fair.
	53	2	0	53	60	30,09	62		E	1	Fine.
9	42,5	7	0	43	57	30,07	72		NE	1	Cloudy.
	49	2	0	49	58	30,05	68		E	1	Cloudy.
10	40	7	0	41	57	29,94	71		S	1	Fair.
	51	2	0	50	59	29,85	71		SSW	2	Cloudy.
11	49	7	0	49	56	29,51	78	0,168	SSW	2	Fair.
	56	2	0	55	60	29,57	57		SW	2	Fair.
12	40	7	0	41	57	29,67	71		SW	1	Cloudy.
	48	2	0	46	57	29,50	70		SW	2	Rain.
13	36	7	0	36	55	29,69	72	0,256	SW	1	Fine.
	49	2	0	46	58	29,81	60		W	2	Cloudy.
14	40	7	0	42	55	30,08	71		SW	1	Cloudy.
	51	2	0	51	57	30,10	65		SW	1	Cloudy.
15	45	7	0	49	56	30,00	78	0,125	SSW	2	Cloudy.
	52	2	0	52	58	29,95	75		SSW	2	Rain.
16	48	7	0	48	56	29,93	77	0,093	SSW	1	Rain.
	50	2	0	50	57	29,93	70		SSW	1	Cloudy.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar. 17	40	7	0	40	56	30,08	74	0,018	W	2	Fair.
	54	2	0	54	58	30,08	59		SW	1	Fine.
18	42	7	0	47	57	29,78	79	0,095	ESE	1	Rain.
	55	2	0	55	59	29,59	70		SSE	2	Cloudy.
19	41	7	0	45	57	29,66	68	0,048	SSW	2	Fair.
	50	2	0	50	58	29,70	63		SSW	2	Fair.
20	38	7	0	40	55	30,05	72		NE	1	Fine.
	49	2	0	49	57	30,19	67		NE	1	Cloudy.
21	36	7	0	40	54	30,44	68		NE	1	Cloudy.
	49	2	0	46	56	30,46	64		E	1	Cloudy.
22	34	7	0	36	54	30,43	69		ENE	2	Fair.
	49	2	0	49	58,5	30,36	64		E	1	Fair.
23	39	7	0	42	55	30,25	74		NE	1	Cloudy.
	54	2	0	54	58	30,21	64		E	1	Fair.
24	43	7	0	44	56	30,25	77		E	1	Cloudy.
	50	2	0	50	58	30,27	70		E	1	Cloudy.
25	41	7	0	43	56	30,30	78		E	1	Cloudy.
	52	2	0	52	59	30,27	73		E	1	Cloudy.
26	40	7	0	43	56	30,21	78		NE	1	Cloudy.
	56	2	0	56	60	30,13	59		ENE	1	Fine.
27	36	7	0	42	55	30,11	75		NE	1	Cloudy.
	50	2	0	50	59	30,11	69		NE	1	Fair.
28	36	7	0	37	55	30,10	73		NE	1	Cloudy.
	54	2	0	54	59,5	30,08	68		NE	1	Fair.
29	43	7	0	47	57	29,87	75		S	1	Cloudy.
	55	2	0	55	60	29,79	66		SSW	1	Cloudy.
30	45	7	0	45	56	29,77	73	0,087	WSW	2	Cloudy.
	53	2	0	53	59	29,83	59		WNW	2	Fair.
31	41	7	0	47	57	29,78	73		SSE	2	Cloudy.
	53	2	0	53	59	29,68	68		SSW	2	Fair.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 1	38	7	0	38	57	29,68	78	0,108	SW	2	Fair.
	53	2	0	52	59	29,50	53		W	2	Fair.
2	43	7	0	43	57	29,63	69		SSW	1	Fine.
	51	2	0	51	59	29,60	68		SSW	2	Cloudy.
3	47	7	0	47	57	29,51	77	0,015	S	2	Rain.
	55	2	0	55	59	29,46	72		S	2	Cloudy.
4	38	7	0	38	57	29,05	73	0,280	S	2	Rain. Much wind
	54	2	0	52	59	29,32	62		SSW	2	Cloudy. [last
5	41	7	0	45	57	29,72	72	0,101	W	2	Fine. night.
	54	2	0	54	61	29,76	52		W	2	Fair.
6	45	7	0	47	58	29,52	71	0,135	WSW	2	Cloudy.
	52	2	0	52	60	29,48	65		SW	2	Cloudy.
7	47	7	0	47	57	29,02	74	0,268	SSW	2	Cloudy.
	56	2	0	55	69	28,98	70		SSW	2	Rain.
8	43	7	0	43	58	29,16	69	0,300	SSW	2	Fair.
	52	2	0	51	58	29,19	63		SSW	2	Cloudy.
9	41	7	0	43	56	29,40	73		NE	1	Fair.
	47	2	0	46	57	29,52	68		NE	2	Cloudy.
10	40	7	0	42	56	29,88	72		NE	2	Cloudy.
	51	2	0	50	57	29,98	68		NE	2	Cloudy.
11	42	7	0	43	56	30,18	69		NW	1	Cloudy.
	51	2	0	51	58	30,16	58		SW	1	Cloudy.
12	43	7	0	43	56	30,10	74	0,110	N	1	Rain.
	51	2	0	51	59	30,15	67		NE	1	Cloudy.
13	43	7	0	44	55	30,11	72		NE	2	Cloudy.
	52	2	0	51,5	57	30,03	66		NE	1	Cloudy.
14	43	7	0	47	56	30,04	70		W	1	Cloudy.
	56	2	0	56	58	30,04	57		WSW	1	Cloudy.
15	46	7	0	47	56	29,85	74	0,057	SSW	1	Cloudy.
	58	2	0	58	60	29,96	52		W	1	Cloudy.
16	43	7	0	45	57	30,20	67		SW	1	Fine.
	58	2	0	58	60	30,29	54		SW	1	Fair.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 17	43	7	0	48	58	30,44	70		SSW	1	Hazy.
	59	2	0	59	60	30,44	59		E	1	Cloudy.
18	43	7	0	46	57	30,34	65		ENE	1	Fine.
	59	2	0	59	61	30,24	63		ENE	1	Hazy.
19	45	7	0	47	58	30,06	73		SW	1	Cloudy.
	62	2	0	62	61	30,04	57		SW	1	Fine.
20	44	7	0	47	59	30,05	70		WSW	1	Fine.
	63	2	0	63	62	30,06	65		WSW	1	Cloudy.
21	49	7	0	52	60	30,13	64		NE	1	Hazy.
	62	2	0	62	62	30,09	65		NE	1	Cloudy.
22	51	7	0	51	60	30,03	69		E	1	Fair.
	66	2	0	66	64	30,00	52		SSE	1	Hazy.
23	53	7	0	57	61	29,91	63		E	1	Cloudy.
	70	2	0	70	64	29,83	54		SE	2	Cloudy.
24	52	7	0	52	62	30,11	63		W	1	Fine.
	65	2	0	65	64	30,20	49		WNW	1	Fine.
25	49	7	0	53	62	30,37	61		SW	1	Fine.
	67	2	0	67	64	30,33	54		S	1	Fine.
26	52	7	0	52	63	30,28	58		SW	1	Fine.
	71	2	0	71	66	30,23	52		SSW	1	Fine.
27	56	7	0	58	64	30,20	63	0,022	E	1	Cloudy.
	73	2	0	71,5	66	30,09	55		S	2	Fair.
28	50	7	0	52	64	30,16	63		WSW	1	Fine.
	64	2	0	62	64	30,05	64		S	2	Cloudy.
29	52	7	0	53	62	30,09	65		SW	2	Cloudy.
	62	2	0	61	63	30,07	63		SW	2	Cloudy.
30	51	7	0	52	62	29,99	60		SSW	1	Cloudy.
	62	2	0	61	62	29,90	55		SSW	2	Fair.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
May 1	50	7	0	52	61	29,78	65		S	2	Cloudy.
	61	2	0	61	62	29,72	57		SE	1	Cloudy.
2	49	7	0	51	61	29,91	65		E	2	Cloudy.
	57	2	0	57	62	30,07	54		E	1	Fair.
3	41	7	0	47	60	30,18	65		E	1	Hazy.
	61	2	0	61	62	30,07	47		SE	1	Fine.
4	46	7	0	51	60	30,07	62		E	1	Fine.
	62	2	0	62	62	30,01	56		E	1	Fine.
5	51	7	0	52	61	29,85	63		E	1	Cloudy.
	59	2	0	58	62	29,82	60		NE	1	Cloudy.
6	43	7	0	48	60	30,00	62		SSW	2	Cloudy.
	57	2	0	56	60	29,94	60		WSW	2	Cloudy.
7	51	7	0	54	59	29,93	65		SW	2	Cloudy.
	63	2	0	61	61	29,90	60		WSW	2	Cloudy.
8	51	7	0	51	59	29,63	63		SSW	2	Cloudy.
	56	2	0	51	59	29,48	64		WSW	2	Cloudy.
9	40	7	0	43	57	29,43	65	0,301	S	2	Fine.
	52	2	0	47	57	29,44	65		SSW	2	Rain.
10	41	7	0	44	57	29,45	65	0,302	SE	1	Fine.
	57	2	0	51	58	29,45	63		SSE	2	Fair.
11	43	7	0	47	57	29,61	64	0,092	S	2	Fine.
	59	2	0	58	59	29,61	51		SE	2	Hazy.
12	46	7	0	47	57	29,66	64		SSW	2	Hazy.
	61	2	0	61	60	29,74	51		SSW	1	Fair.
13	47	7	0	47	58	29,90	64		NE	1	Cloudy.
	58	2	0	56	58	29,97	61		NW	1	Cloudy.
14	44	7	0	46	57	30,30	68	0,288	E	1	Fine.
	68	2	0	68	60	30,36	52		WSW	1	Fair.
15	53	7	0	55	58,5	30,45	68		SW	1	Cloudy.
	71	2	0	71	61	30,45	57		W	1	Fine.
16	55	7	0	56	60	30,57	68		NE	1	Fair.
	70	2	0	66	62	30,58	53		E	1	Fair.

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May 17	52	7 0	56	60	30,52	67		SW	1	Cloudy.
	71	2 0	71	63	30,41	56		SSE	1	Fine.
18	54	7 0	57	62	30,25	67		W	1	Hazy.
	63	2 0	62	62	30,19	60		WNW	1	Cloudy.
19	52	7 0	56	62	30,19	68		NE	1	Cloudy.
	62	2 0	62	62	30,22	56		NE	1	Cloudy.
20	49	7 0	52	60	29,96	67		WNW	1	Cloudy.
	58	2 0	58	59	29,89	57		NW	1	Cloudy.
21	42	7 0	47	60	30,01	63	0,113	NW	1	Fine.
	56	2 0	56	60	29,96	56		NW	1	Cloudy.
22	46	7 0	50	58,5	29,82	67		NW	1	Cloudy.
	59	2 0	54	59	29,88	65		N	1	Rain.
23	41	7 0	45	57	30,06	65		WNW	1	Fine.
	61	2 0	61	59	29,97	57		W	1	Cloudy.
24	47	7 0	52	58	29,92	64	0,032	WNW	1	Cloudy.
	57	2 0	55	58	29,91	62		N	1	Cloudy.
25	42	7 0	47	57,5	29,84	65	0,152	NE	1	Cloudy.
	55	2 0	55	57,5	29,85	54		NE	2	Cloudy.
26	41	7 0	46	56,5	29,86	66		NE	1	Cloudy.
	53	2 0	53	57	29,83	57		NE	1	Cloudy.
27	42	7 0	47	56	29,75	84	0,200	NE	1	Rain.
	50	2 0	49	56,5	29,73	89		NE	1	Rain.
28	46	7 0	48	56	29,74	74	0,735	NW	1	Cloudy.
	55	2 0	54	57	29,74	62		NW	1	Cloudy.
29	48	7 0	52	56	29,90	71		NE	1	Cloudy.
	64	2 0	64	58	30,01	59		NE	1	Cloudy.
30	48	7 0	52	57	30,21	65		WNW	1	Hazy.
	64	2 0	60	59	30,24	58		NW	1	Cloudy.
31	51	7 0	53	58	30,30	64		NE	1	Cloudy.
	63	2 0	61	60	30,30	55		NW	1	Fine.

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June 1	46	7	0	48	58	30,33	62		NE	1	Cloudy.
	58	2	0	58	59	30,30	57		NE	1	Fair.
2	46	7	0	51	58	30,25	66		NE	1	Cloudy.
	66	2	0	66	60	30,21	51		E	1	Fine.
3	50	7	0	52	59,5	30,18	68		NE	1	Cloudy.
	58	2	0	56	60	30,15	64		NE	1	Cloudy.
4	48	7	0	51	58,5	30,15	68		NE	1	Cloudy.
	60	2	0	60	60	30,15	59		NE	1	Cloudy.
5	48	7	0	51	58	30,16	68		NE	1	Cloudy.
	54	2	0	54	58	30,13	65		NE	1	Cloudy.
6	47	7	0	50	58	30,02	62		NE	1	Cloudy.
	67	2	0	67	61	29,90	51		W	1	Fine.
7	52	7	0	56	60	29,70	64		NE	1	Cloudy.
	60	2	0	59	60	29,71	60		NE	1	Cloudy.
8	47	7	0	51	59	29,92	58		NE	1	Fair.
	64	2	0	61	60	29,91	51		NE	1	Fair.
9	47	7	0	52	60	29,92	61		NW	1	Cloudy.
	63,5	2	0	63,5	60,5	29,99	54		NW	1	Cloudy.
10	55	7	0	57	60	30,00	59		W	1	Fine.
	67	2	0	66	61	29,99	55		WSW	1	Cloudy.
11	49	7	0	53	60	29,95	63		SW	1	Cloudy.
	66	2	0	65	62	29,95	56		SSW	1	Fair.
12	50	7	0	56	61	30,00	64		SW	1	Fair.
	69	2	0	67,5	62,5	30,01	57		SSW	1	Fine.
13	53	7	0	57	62	30,00	64		SE	1	Fine.
	74	2	0	73	63	29,90	57		ESE	1	Fine.
14	60	7	0	64	65	29,88	70	0,337	E	1	Fair.
	71	2	0	70	67	29,97	61		W	1	Cloudy.
15	54	7	0	58	65	30,16	67		NE	1	Fair.
	67	2	0	66	66	30,16	56		ENE	1	Fair.
16	50	7	0	56	55	30,20	60		E	1	Fine.
	66	2	0	66	66	30,18	51		E	1	Fine.

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June 17	47	7	0	55	62	30,20	62		E	1	Fair.
	64,5	2	0	64,5	66	30,19	55		E	1	Fine.
18	50	7	0	53	64	30,13	62		E	1	Cloudy.
	71	2	0	70	65,5	30,07	54		E	1	Fine.
19	53	7	0	55	64	29,90	67		NE	1	Cloudy.
	60	2	0	59	63	29,85	65		NE	1	Cloudy.
20	57	7	0	59	64	29,71	65		SW	1	Cloudy.
	69	2	0	68	65	29,69	55		SW	1	Cloudy.
21	52	7	0	57	64	29,85	66		NE	1	Cloudy.
	69	2	0	67	65	29,92	57		NE	1	Cloudy.
22	56	7	0	58	64	30,07	65		E	1	Fine.
	74	2	0	74	66	30,07	53		E	1	Fine.
23	50	7	0	56	65	30,03	65		E	1	Cloudy.
	75	2	0	75	66	29,96	54		E	1	Fine.
24	59	7	0	63	66	29,91	58		SW	1	Hazy.
	79	2	0	79	68	29,84	50		SSW	1	Fine.
25	57	7	0	61	67	29,84	59	0,048	NE	1	Cloudy.
	69	2	0	69	67	29,84	54		NE	1	Cloudy.
26	59	7	0	62	66	29,90	57		NE	1	Fair.
	74	2	0	72	68	29,93	48		NE	1	Fair.
27	54	7	0	60	66	30,21	63		NE	1	Cloudy.
	72	2	0	70	68	30,26	45		WNW	1	Fine.
28	54	7	0	59	67	30,34	59		SE	1	Fine.
	78	2	0	76	69	30,30	47		SSE	1	Fine.
29	54	7	0	61	67	30,29	58		E	1	Fine.
	69	2	0	68,5	69	30,24	51		E	1	Fine.
30	56	7	0	62	68	30,03	62		E	1	Fine.
	75	2	0	75	70	29,96	54		ESE	1	Fair.

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July 1	57	7	0	61	68	29,97	63		WSW	1	Cloudy.
	75'	2	0	75	69	30,00	55		NW	1	Fair.
2	57	7	0	61	68	30,15	61		WSW	1	Fair.
	76	2	0	75	70	30,15	51		NW	1	Fine.
3	63	7	0	67	70	30,12	64		SW	1	Cloudy.
	79	2	0	77	71	30,12	55		WNW	1	Cloudy.
4	58	7	0	62	70	30,26	57		NE	1	Fair.
	76	2	0	76	72	30,26	53		E	1	Hazy.
5	59	7	0	62	70	30,22	59		E	1	Fine.
	72	2	0	72	72	30,17	51		E	1	Fine.
6	56	7	0	64	70	30,00	64		E	1	Fine.
	79	2	0	79	73	29,90	54		E	1	Fine.
7	64	7	0	67	72	29,90	63		NE	1	Hazy.
	83	2	0	83	73,5	29,94	54		SSW	1	Fine.
8	58	7	0	61	71	30,12	60		W	1	Fair.
	81	2	0	80	74	30,12	51		SSW	1	Fine.
9	62	7	0	66	73	30,27	59		NE	1	Hazy.
	79	2	0	79	74	30,28	56		E	1	Hazy.
10	59	7	0	64	72	30,37	60		E	1	Cloudy.
	72	2	0	72	72	30,30	56		E	1	Cloudy.
11	58	7	0	64	72	30,15	59		E	1	Fair.
	80	2	0	79	73	30,07	49		NE	1	Fine.
12	58	7	0	64	72	30,04	57		E	1	Fine.
	75	2	0	73	73	30,01	51		E	1	Fine.
13	58	7	0	63	72	29,94	62		E	1	Fine.
	84	2	0	84	73	29,90	50		E	1	Fine.
14	61	7	0	63	73	30,02	61		WNW	1	Fine.
	77	2	0	77	73	30,07	49		WNW	1	Cloudy.
15	60	7	0	63	72	30,16	60		SW	1	Cloudy.
	76	2	0	75	73	30,12	51		SW	1	Cloudy.
16	61	7	0	65	72	30,05	60		SW	1	Cloudy.
	74	2	0	73	72	30,05	59		S	2	Cloudy.

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July 17	60	7	0	62	72	30,06	62		SSW	1	Cloudy.
	78	2	0	78	72	30,06	52		S	1	Hazy.
18	65	7	0	66	72	30,06	58		E	1	Cloudy.
	78	2	0	77	73	30,06	54		E	1	Hazy.
19	67	7	0	69	73	30,00	62		SW	1	Hazy.
	81	2	0	80	74,5	29,96	52		NW	1	Fine.
20	61	7	0	64	73	29,95	58		S	1	Fair.
	79,5	2	0	79	73,5	29,90	52		SW	2	Fair.
21	58	7	0	60	68	29,88	56		SW	2	Fair.
	72	2	0	71	72	29,93	46		W	2	Fair.
22	57	7	0	61	70	29,91	54		S	1	Cloudy.
	75	2	0	74	72	29,80	50		SSE	2	Fair.
23	64	7	0	65	71	29,58	59	0,067	SSE	2	Fair.
	72	2	0	70	70	29,54	53		S	2	Fair.
24	59	7	0	60,5	69,5	29,47	64	0,097	SW	2	Cloudy.
	73	2	0	73	71	29,59	55		SW	2	Fair.
25	54	7	0	57	69,5	29,89	57	0,122	W	1	Fine.
	71	2	0	71	70	29,89	56		SW	1	Cloudy.
26	59	7	0	60	68	29,87	62	0,091	SSE	1	Cloudy.
	72	2	0	72	69,5	29,80	58		SSE	2	Cloudy.
27	58	7	0	60	68	29,92	61		SW	1	Cloudy.
	70,5	2	0	70	69	29,92	53		W	1	Cloudy.
28	59	7	0	61	68	29,91	64	0,138	SW	1	Cloudy.
	72,5	2	0	72,5	69	29,89	57		SSW	1	Cloudy.
29	60	7	0	63	68	29,91	64		SSW	1	Cloudy.
	79	2	0	78	70	29,91	57		SSW	1	Fair.
30	65	7	0	67	70	29,91	64		S	1	Fair.
	81,5	2	0	81	72	29,88	54		SW	1	Fair.
31	63	7	0	65	68	29,88	66		SW	1	Cloudy.
	75	2	0	75	72	29,87	55		WSW	1	Fair.

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Aug. 1	60	7	0	63	70	29,68	58		SSE	2	Cloudy.
	72	2	0	72	71	29,56	59		SSE	2	Fair.
2	59	7	0	62	70	29,51	62	0,030	SSW	2	Cloudy.
	70	2	0	70	71	29,58	58		W	2	Fair.
3	59	7	0	61	68	29,77	56	0,148	WSW	2	Fair.
	66	2	0	66	68	29,76	56		SSW	2	Cloudy.
4	57	7	0	60	67	29,69	59		WNW	2	Cloudy.
	64	2	0	64	68	29,75	51		NW	2	Cloudy.
5	50	7	0	56	66	29,80	58		NW	2	Cloudy.
	68	2	0	66	66,5	29,78	54		WNW	1	Cloudy.
6	55	7	0	58	66	29,63	66	0,063	E	1	Cloudy.
	69	2	0	68	67	29,63	61		SW	1	Cloudy.
7	57	7	0	61	66	29,61	68	0,117	E	1	Cloudy.
	72	2	0	70	67,5	29,56	59		SSE	1	Cloudy.*
8	55	7	0	57	66	29,79	66	0,410	NW	2	Cloudy.
	63	2	0	62	67	29,89	59		N	2	Cloudy.
9	49	7	0	54	65	30,14	61		N	1	Fair.
	65	2	0	65	66	30,16	55		N	1	Cloudy.
10	53	7	0	55	65	30,16	61		W	1	Cloudy.
	67	2	0	64	65	30,15	57		W	1	Cloudy.
11	57	7	0	59	65	30,05	70		SW	1	Cloudy.
	76	2	0	73	67	30,05	60		WSW	1	Fair.
12	57	7	0	59	66	30,19	65		NNE	1	Fair.
	71	2	0	70	67,5	30,24	52		N	1	Fine.
13	53	7	0	55	67	30,28	64		N	1	Fine.
	73	2	0	73	68	30,24	52		E	1	Hazy.
14	54	7	0	57	67	30,16	65		E	1	Hazy.
	74	2	0	74	69	30,08	52		E	1	Fine.
15	57	7	0	59	68	29,96	63		E	1	Hazy.
	75	2	0	73	69	29,89	53		E	1	Fair.
16	59	7	0	61	68	29,86	61	0,041	S	1	Fair.
	73	2	0	71	69	29,86	56		SW	1	Cloudy.

* A violent storm of rain and hail, with thunder & lightning.

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Aug. 17	60	7	0	63	68,5	29,89	61		S	1	Fair.
	78	2	0	77	70	29,90	53		S by W	1	Fine.
18	59	7	0	61	69,5	29,98	65		SW	1	Hazy.
	74	2	0	72	70	29,99	51		W	1	Fine.
19	56	7	0	57	68	29,98	65		WSW	1	Hazy.
	74	2	0	72	69,5	29,96	53		SW	1	Hazy.
20	55	7	0	57	68	30,04	64		NW	1	Cloudy.
	72	2	0	71	69	30,05	53		NNE	1	Cloudy.
21	53	7	0	55	67	30,18	63		N	1	Cloudy.
	70	2	0	69	69	30,14	52		NW	1	Cloudy.
22	51	7	0	53	67	30,18	63		W	1	Hazy.
	70	2	0	68	68	30,14	52		NW	1	Fair.
23	58	7	0	60	67,5	30,02	74	0,088	SW	1	Rain.
	72	2	0	72	69	29,97	60		WSW	1	Cloudy.
24	52	7	0	53	67	29,98	62	0,022	SW	1	Fine.
	73	2	0	73	68	29,96	51		SSW	1	Fine.
25	58	7	0	59	67	29,78	63		SSE	2	Cloudy.
	71	2	0	70	68,5	29,75	60		SSW	2	Fair.
26	53	7	0	53	67	29,88	64	0,243	SSW	1	Hazy.
	71	2	0	68	68	29,86	53		SSW	1	Cloudy.
27	54	7	0	54	66	29,71	66	0,225	SW	2	Cloudy.
	63	2	0	61	66	29,79	61		WSW	2	Cloudy.
28	50	7	0	53	65	29,91	64	0,113	WNW	2	Cloudy.
	67	2	0	66	66	29,98	52		NW	2	Cloudy.
29	48	7	0	52	64,5	30,04	64		SW	1	Hazy.
	68	2	0	68	66	29,99	51		S	1	Hazy.
30	57	7	0	59	65	29,84	66	0,015	ESE	1	Cloudy.
	69	2	0	68	68	29,78	62		S	1	Cloudy.
31	58	7	0	60	65	29,67	73	0,090	S	1	Rain.
	67	2	0	66	66	29,67	67		S	1	Fair.

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Sep. 1	53	7	0	56	65	29,92	69	0,063	SSW	1	Cloudy.
	68	2	0	65	66	29,92	59		SSW	1	Cloudy.
2	57	7	0	57	65	29,80	68		W	1	Fine.
	66	2	0	65	66,5	29,87	62		NW	2	Cloudy.
3	54	7	0	56	65	30,11	66		NE	2	Fine.
	66	2	0	65	66	30,18	63		NE	1	Cloudy.
4	48	7	0	50	64	30,18	68		NE	1	Fine.
	65	2	0	65	65	30,13	54		S	1	Fair.
5	56	7	0	57	64	29,90	73	0,120	E	1	Cloudy.
	62	2	0	58	64,5	29,78	65		ESE	1	Rain.
6	53	7	0	54	64	29,57	75	0,442	NE	1	Rain.
	62	2	0	61	64	29,53	66		NE	1	Fair.
7	53	7	0	54	63	29,53	71	0,015	NE	1	Cloudy.
	64	2	0	63	65	29,56	64		NE	1	Cloudy.
8	53	7	0	54	63,5	29,72	82	0,360	NE	1	Rain.
	60	2	0	57	64	29,72	84		NE	1	Rain.
9	55	7	0	56	63,5	29,72	80	0,236	NE	1	Rain.
	61	2	0	58	64	29,77	78		NE	1	Rain.
10	53	7	0	56	62,5	29,88	84	1,020	NE	1	Cloudy.
	61	2	0	61	63	29,98	68		NE	1	Cloudy.
11	48,5	7	0	51	62	30,10	83	0,146	NE	1	Fine.
	61	2	0	60	63	30,14	58		NE	1	Cloudy.
12	49	7	0	53	62	30,23	71		NE	1	Cloudy.
	60	2	0	60	62	30,25	56		NE	1	Cloudy.
13	52	7	0	52	61,5	30,12	59		NE	1	Cloudy.
	57	2	0	57	61,5	30,03	52		NE	1	Cloudy.
14	50	7	0	51	60,5	29,87	66		NE	1	Cloudy.
	57	2	0	57	61	29,85	63		NE	1	Cloudy.
15	48	7	0	50	60	29,82	71		NE	1	Cloudy.
	59	2	0	59	61	29,83	60		NE	1	Cloudy.
16	52	7	0	56	60	29,89	68		SSE	2	Cloudy.
	66	2	0	66	62	29,91	65		SSW	2	Cloudy.

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Sep. 17	58	7	0	59	61,5	29,88	75	0,041	S	2	Cloudy.
	67	2	0	67	64	29,82	69		S	2	Fair.
18	59	7	0	56	62	29,51	73	0,041	S	1	Rain.
	63	2	0	63	64	29,51	57		S	2	Fair.
19	50	7	0	53	63	29,66	69		SE	1	Hazy.
	64	2	0	63	64,5	29,59	57		S	2	Fair.
20	53	7	0	52,5	62	29,27	68	0,170	S	2	Cloudy.
	60	2	0	59	63	29,24	61		S	2	Fair.
21	48	7	0	50	61,5	29,64	68	0,042	SW	2	Fine.
	62	2	0	62	62	29,73	65		W	2	Cloudy.
22	53	7	0	56	62	29,79	72		SE	1	Cloudy.
	66,5	2	0	66,5	63	29,76	65		S	2	Cloudy.
23	60	7	0	61	63	29,54	71		SSW	2	Cloudy.
	65	2	0	65	65	29,42	63		SSW	2	Fair.
24	53	7	0	55	63	29,35	73	0,187	E	2	Cloudy.
	57	2	0	56	63	29,37	68		WNW	1	Rain.
25	45	7	0	47	62	29,58	69	0,094	W	1	Cloudy.
	58	2	0	54	62	29,65	65		NW	1	Rain.
26	42	7	0	44	61	29,86	66	0,035	NW	1	Cloudy.
	52	2	0	52	62	29,93	60		N	1	Cloudy.
27	41	7	0	42	59,5	30,06	69		NW	1	Fair.
	61	2	0	60,5	62	30,08	54		NW	1	Fine.
28	37	7	0	39	58	30,20	64		W	1	Fair.
	52	2	0	52	61	30,22	56		NW	1	Fine.
29	44	7	0	46	59	30,32	69		ESE	1	Cloudy.
	56	2	0	56	60	30,33	65		SE	1	Cloudy.
30	48	7	0	48	59	30,36	69		W	1	Cloudy.
	61	2	0	60	61	30,33	62		N	1	Cloudy.

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1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Oct. 1	50	7	0	51	59,5	30,30	74	0,035	E	1	Cloudy.
	56	2	0	56	59,5	30,26	68		SW	1	Cloudy.
2	52	7	0	54	60	30,18	78	0,046	SW	1	Cloudy.
	57	2	0	56,5	61,5	30,17	69		SW	1	Cloudy.
3	53	7	0	53	60	30,09	76	0,060	E	1	Cloudy.
	58	2	0	58	62,5	30,00	67		S	1	Cloudy.
4	48	7	0	51	60,5	29,82	71	0,026	SE	1	Cloudy.
	56	2	0	55	62	29,79	70		NW	1	Rain.
5	42	7	0	43	59,5	29,89	74	0,054	W	1	Fair.
	56	2	0	52	60	29,74	69		SSW	1	Rain.
6	48	7	0	49	58	29,15	68	0,272	SW	2	Cloudy.
	54	2	0	54	60	29,29	57		WSW	2	Fair.
7	41	7	0	42	57,5	29,66	68		WSW	1	Fine.
	55	2	0	53,5	59,5	29,65	63		WSW	1	Fair.
8	47	7	0	47	58	29,35	70	0,203	W	1	Fair.
	55	2	0	52,5	61	29,45	57		NW	2	Fair.
9	43	7	0	44	57,5	29,76	66	0,053	WNW	1	Fair.
	54,5	2	0	54,5	59,5	29,83	57		NW	1	Cloudy.
10	49	7	0	54	59	29,71	82	0,020	S	2	Cloudy.
	63	2	0	63	62	29,66	63		S	2	Cloudy.
11	57,5	7	0	58	61	29,43	77	0,045	S	2	Cloudy.
	61	2	0	60	63	29,54	60		SSW	2	Fair.
12	46	7	0	47	60	29,90	68	0,038	SW	1	Fine.
	57,5	2	0	57,5	62	29,93	55		SW	1	Fine.
13	39	7	0	40	59,5	29,93	67		NE	1	Fine.
	55	2	0	54,5	62	29,83	60		NE	1	Fine.
14	50	7	0	53	61	29,48	86	0,110	E	1	Rain.
	60,5	2	0	60	63	29,57	63		SSW	2	Fine.
15	54	7	0	54,5	61	29,86	78	0,177	SSW	1	Fair.
	63	2	0	62	65	30,01	60		SSW	1	Fair.
16	54	7	0	54	62	30,16	80		SE	1	Cloudy.
	62	2	0	61,5	63,5	30,14	63		SE	1	Fair.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Oct. 17	52	7	0	53	62	30,07	78		W	1	Cloudy.
	61	2	0	60	64	29,97	63		S	2	Hazy.
18	44	7	0	45	62	29,85	70	0,183	W	2	Fine.
	57	2	0	53	64	29,86	62		W	1	Fair.
19	42	7	0	43	59	30,08	71	0,081	WNW	1	Fine.
	51	2	0	51	61	30,08	67		W	1	Rain.
20	49	7	0	49	60	30,12	78	0,127	E	1	Cloudy.
	56,5	2	0	56,5	62	30,16	73		S	1	Cloudy.
21	48	7	0	48	60,5	30,34	77	0,072	NE	1	Cloudy.
	53	2	0	53	62	30,34	65		NNE	1	Fair.
22	48	7	0	49	60	30,17	76		E	1	Fair.
	55	2	0	54	61	30,04	65		ESE	1	Cloudy.
23	44	7	0	44	60	29,76	73	0,627	WNW	1	Cloudy.
	48	2	0	47	60	29,76	68		N	1	Cloudy.
24	44	7	0	45	58	29,79	74	0,220	N	2	Cloudy.
	49	2	0	48	58	29,83	70		N	2	Cloudy.
25	38	7	0	40	57,5	29,91	70		NE	1	Fair.
	50,5	2	0	50,5	60,5	29,91	67		NE	1	Fair.
26	42	7	0	44	57,5	29,79	75		W	1	Cloudy.
	53	2	0	53	59	29,75	74		SW	1	Cloudy.
27	50	7	0	50	58	29,46	80	0,076	SSW	1	Rain.
	50	2	0	50	59	29,29	76		W	1	Rain.
28	40	7	0	40	57	29,35	73	0,261	W	1	Cloudy.
	45	2	0	44	57	29,34	72		N	1	Cloudy.
29	35	7	0	36	56	29,68	73		SW	1	Fine.
	47	2	0	47	58	29,73	61		WNW	1	Fine.
30	39	7	0	44	55	29,41	78	0,056	SW	1	Rain.
	51	2	0	51	58	29,51	66		NW	1	Fair.
31	44	7	0	45	56,5	29,80	74		W	1	Cloudy.
	53	2	0	53	58	29,83	78		SW	1	Rain.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Nov. 1	50	7	0	50	57	29,92	76	0,026	WSW	1	Fair.
	57	2	0	56	59,5	29,86	64		WSW	1	Cloudy.
2	46	7	0	48	57	29,71	77	0,056	SW	1	Cloudy.
	52	2	0	50	59	29,57	75		SW	1	Rain.
3	44	7	0	45	57,5	29,20	75	0,360	SW	1	Cloudy.
	46	2	0	46	58	29,24	69		WNW	1	Fair.
4	40	7	0	42	56,5	29,29	73		SW	1	Cloudy.
	53	2	0	48	58	29,11	75		SSE	1	Rain.
5	49	7	0	54	58	29,16	74	0,223	S	3	Cloudy.
	56	2	0	53	59	29,18	77		S	2	Rain.
6	47	7	0	49	58	29,47	76	0,385	S	1	Rain.
	49	2	0	49	60	29,50	70		S	1	Rain.
7	40	7	0	41	58	29,52	76	0,555	SSE	1	Fair.
	50	2	0	50	59	29,47	75		SE	1	Rain.
8	44	7	0	45	58	29,58	75	0,235	SSE	2	Cloudy.
	50	2	0	50	58	29,73	68		S	2	Cloudy.
9	35	7	0	35	56,5	29,98	75		WNW	1	Fair.
	46	2	0	45	58	30,05	72		WNW	1	Fair.
10	36	7	0	37	55,5	30,15	75		N	1	Fine.
	52	2	0	50	57	30,06	68		SSE	1	Cloudy.
11	52	7	0	52	56,5	29,90	80		SSW	1	Cloudy.
	56	2	0	56	58	29,95	76		SSW	1	Cloudy.
12	46	7	0	47	57	30,02	74	0,028	WNW	1	Rain.
	49	2	0	49	58	30,00	72		NNW	1	Cloudy.
13	36	7	0	37	56,5	30,02	69		NW	1	Fine.
	44	2	0	44	57,5	30,03	63		NW	1	Fine.
14	36	7	0	40	56	30,17	70		NW	1	Cloudy.
	46	2	0	46	57	30,19	63		NW	1	Cloudy.
15	42	7	0	44	56	30,18	77		NW	1	Foggy.
	51	2	0	51	58	30,03	76		W	1	Cloudy.
16	44	7	0	48	57	29,92	76		WSW	1	Cloudy.
	52	2	0	52	57	29,95	58		WSW	1	Cloudy.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Nov. 17	41	7	0	42	57	30,10	70		NE	1	Cloudy.
	43	2	0	43	57	30,10	66		NE	1	Cloudy.
18	34	7	0	34	54	30,05	63		ESE	1	Cloudy.
	37	2	0	37	53	29,97	62		ESE	1	Cloudy.
19	31	7	0	31,5	51,5	29,73	65		E	2	Fair.
	36	2	0	36	52	29,61	60		E	2	Fair.
20	30,5	7	0	31,5	49	29,36	68		E	2	Fair.
	36	2	0	36	50,5	29,20	68		E	2	Cloudy.
21	37	7	0	46	51	29,22	83	0,200	SE	2	Cloudy.
	51	2	0	51	54	29,38	75		SE	2	Cloudy.
22	43	7	0	43	52	29,68	78	0,050	E	1	Cloudy.
	49	2	0	48,5	55	29,73	78		E	1	Fair.
23	44	7	0	44	53	29,77	80		E	1	Cloudy.
	47	2	0	46	56	29,74	78		E	1	Cloudy.
24	43	7	0	48	55	29,58	89	0,396	E	1	Foggy.
	54	2	0	53	57	29,63	88		SW	1	Fair.
25	46	7	0	48	56	29,74	87	0,115	SW	1	Cloudy.
	54	2	0	51	57,5	29,74	79		S	1	Cloudy.
26	44	7	0	44	56,5	29,60	73	0,030	SW	1	Fair.
	49	2	0	48	58	29,76	61		SW	1	Fine.
27	36	7	0	40	55	29,85	75	0,085	SSW	1	Rain.
	53	2	0	53	57	29,92	63		W	1	Fine.
28	44	7	0	44	55	29,82	72		SSW	2	Cloudy.
	48	2	0	48	56	29,48	80		SSE	2	Rain.
29	43	7	0	43	55	29,60	75	0,265	SW	2	Cloudy.
	46	2	0	46	57	29,85	65		SW	2	Fine.
30	44,5	7	0	47	56	29,84	77	0,331	SSW	2	Fair.
	51	2	0	51	58	29,88	74		S	2	Cloudy.

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1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 1	45	8	0	45	57	29,74	76	0,480	SW	1	Cloudy.
	48	2	0	48	58,5	29,69	73		S	1	Cloudy.
2	39	8	0	47	55	29,93	63		S	2	Cloudy.
	51	2	0	51	57,5	29,85	71		S	2	Cloudy.
3	52	8	0	52	58	29,84	73		S	2	Cloudy.
	54	2	0	52,5	59,5	29,80	67		S	2	Cloudy.
4	46	8	0	47	57,5	29,72	77		S	2	Fair.
	50	2	0	50	60	29,70	73		S	2	Fair.
5	40	8	0	43	57,5	29,67	76		E	1	Foggy.
	48	2	0	48	60	29,70	75		E	1	Fine.
6	44	8	0	45	58,5	29,84	76		E	1	Foggy.
	52	2	0	51	61	29,88	75		S	1	Cloudy.
7	44	8	0	46	58	29,83	78		S	1	Cloudy.
	50	2	0	49	60	29,78	76		S	1	Cloudy.
8	46	8	0	47	59	29,55	77	0,045	SE	1	Rain.
	51	2	0	50	60	29,49	73		SE	1	Fair.
9	39	8	0	42	57	29,65	72		SSE	2	Fair.
	50	2	0	49	59	29,55	75		S	2	Hazy.
10	40	8	0	41	58	29,77	76	0,073	SSW	1	Cloudy.
	42	2	0	42	58	29,92	72		SSW	1	Fair.
11	32	8	0	32	56	30,21	73		W	1	Cloudy.
	42	2	0	42	57	30,22	73		SW	1	Hazy.
12	35	8	0	40	55	30,24	74		SW	1	Cloudy.
	46	2	0	45	57,5	30,24	75		SW	1	Cloudy.
13	39,5	8	0	40	55	30,19	73		S	1	Fair.
	43	2	0	42	57	30,16	69		S	1	Fine.
14	39	8	0	40	55	30,13	73		S	1	Cloudy.
	41	2	0	41	57	30,18	76		S	1	Rain.
15	35,5	8	0	36	55	30,19	77	0,140	E	1	Cloudy.
	36,5	2	0	36	56	30,18	76				Foggy.
16	29	8	0	32	53	30,39	76				Foggy.
	37	2	0	38	55,5	30,44	75		E	1	Fair.

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1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Dec. 17	34	8	0	36	54	30,38	77		S	1	Cloudy.
	41,5	2	0	41,5	55	30,21	75		S b. W	1	Fair.
18	31	8	0	31	53	30,18	75		ESE	1	Fine.
	36	2	0	34,5	54	30,11	73		SSE	1	Fine.
19	30	8	0	30,5	52	29,96	72		ESE	1	Fine.
	37	2	0	37	55	29,94	73		ESE	1	Fine.
20	31	8	0	31	50,5	29,96	71		E	1	Fine.
	34,5	2	0	34,5	53	29,98	70		E	1	Fine.
21	27,5	8	0	28,5	50	29,97	72		E	1	Fine.
	34	2	0	34	52	29,95	71		E	1	Hazy.
22	30	8	0	30	49,5	29,76	74		ENE	1	Cloudy.
	33,5	2	0	33,5	51	29,66	73		ENE	1	Cloudy.
23	33	8	0	37	49	29,72	77		E	1	Cloudy.
	40	2	0	40	52	29,72	75		E	1	Cloudy.
24	29	8	0	29	49	29,82	68		ENE	2	Cloudy.
	30	2	0	29	48	29,84	65		NE	2	Cloudy.
25	25,5	8	0	27	46,5	29,68	73		NE	1	Snow.
	30,5	2	0	30	49	29,60	71		NE	1	Cloudy.
26	27,5	8	0	28	46	29,50	76		NE	1	Snow.
	32	2	0	32	48	29,52	77		NE	1	Snow.
27	26,5	8	0	32	46,5	29,80	79		NE	1	Snow.
	36	2	0	36	49	29,88	79		NE	1	Cloudy.
28	34	8	0	34	47	30,12	78	0,283	NE	1	Rain.
	36,5	2	0	36,5	48	30,18	74		NE	1	Cloudy.
29	33,5	8	0	34	47	30,23	72		NE	1	Cloudy.
	37	2	0	36	49	30,24	69		NE	1	Cloudy.
30	32	8	0	32	47,5	30,18	74		WNW	1	Cloudy.
	35	2	0	35	50	30,14	73		WNW	1	Cloudy.
31	26	8	0	26	47	30,08	72		NW	1	Cloudy.
	32	2	0	32	48,5	30,08	75		NE	1	Cloudy.

1794.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	51	22	35,2	50	22,5	35,6	54	43	48,6	30,56	28,75	30,03	88	58	74,3	0,403
February	56	35	46,7	56	36	47,0	62	51	56,8	30,29	29,40	29,85	80	58	71,8	0,655
March	56	34	46,0	56	36	46,9	60	54	57,1	30,46	29,50	29,98	79	56	69,6	1,077
April	73	38	52,1	71,5	38	52,8	66	55	59,7	30,44	28,98	29,90	77	49	64,4	1,396
May	71	40	53,3	71	43	54,5	63	56	59,2	30,58	29,43	29,96	89	47	62,4	2,215
June	79	46	60,1	79	48	61,5	70	55	63,2	30,34	29,70	30,03	70	45	59	0,385
July	84	54	68,2	84	57	69,4	74,5	68	71,2	30,37	29,47	29,99	66	46	56,9	0,515
August	78	48	62,8	77	52	63,7	71	64,5	67,4	30,28	29,51	29,91	74	51	59,8	1,605
September	68	37	56,1	67	39	56,4	66,5	59	62,6	30,36	29,24	29,85	84	52	66,8	3,012
October	63	35	50,6	63	36	50,8	65	55	60,0	30,34	29,34	29,81	86	55	70,0	2,842
November	57	30,5	45,2	56	31,5	45,7	60	49	56,2	30,19	29,11	29,73	89	58	72,9	3,340
December	54	25,5	38,1	52,5	27	38,7	61	46	53,8	30,44	29,49	29,94	79	63	73,7	1,021
Whole year			51,2			51,9			59,6			29,91			66,8	18,466

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCV.

PART II.

LONDON,

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PHILOSOPHICAL

TRANSACTIONS.

IX. *Some Observations on the Mode of Generation of the Kanguroo, with a particular Description of the Organs themselves. By* Everard Home, *Esq. F. R. S.*

Read March 5, 1795.

THE exertions of the most acute and skilful anatomists have hitherto failed, to explore the process of generation in the quadruped, fully to its origin: I think I may assert, they have ascertained that the embryo comes from the ovarium, and is deposited in the uterus, where it acquires a visible form; but the state in which it leaves the ovarium, the changes it undergoes in the fallopian tube, and its appearance when received by the uterus, are hitherto altogether unknown.

Although we are obliged to confess ourselves ignorant of many things respecting the commencement of generation, the progress of the young from its first visible appearance till it acquires a perfect form, has been very accurately traced; but this may be considered as more properly belonging to the œconomy of the young than to the history of generation itself.

support peculiar to itself. It therefore appears to form a link in the gradation leading from the one to the other.

The American opossum, which is a small animal, was the only one of this tribe that was known in Europe before the late discoveries in the South Seas; and as it had not been found to breed either in France or England, the only accounts of its mode of generation were those received from America, which were vague, and could not be entirely depended on.

These accounts, however, led anatomists, who had opportunities of dissecting the female organs, to endeavour by that method to throw some light upon the subject; but the parts were found to be so complex, and in so many respects different from those of other quadrupeds, that nothing satisfactory could thus be made out, while deprived of an opportunity of seeing them in an impregnated state.*

The discovery of the kanguroo, an animal of a very large size, related in many important points to the opossum, opened a prospect of something more satisfactory being ascertained respecting the generation of these animals; and from the time that a colony was established in New South Wales, it became an inquiry to which several persons directed their attention.

The late Mr. Hunter had for many years kept American opossums, with the sole view of investigating this subject; but

* In Buffon's *Histoire Naturelle* there is an anatomical description of the female organs of the opossum, by Daubenton, and quotations from an account published in England by Tyson, from which he differs in some particulars; but candidly confesses himself not satisfied upon the subject, being unable to make out the uses of the parts. Tyson says there are two ovaria, two tubæ fallopianæ, two uteri, two cornua uteri, two vaginæ uteri. Buff. *Hist. Natur.* Tom. X. p. 302.

was never able to induce them to breed, although all means in his power were employed for that purpose.

This disappointment did not at all abate his ardour; but finding that little was to be expected in that way, he applied to Captain PATERSON, and Mr. LANG a surgeon, who were going to Port Jackson, having received appointments on that establishment, to give him their assistance. He requested they would procure the female organs of the kanguroo under all the different circumstances in which they occurred, and send them to England in spirits, that he might be enabled to prosecute this inquiry. The only preparations of this kind which arrived before Mr. HUNTER's death, were such as shewed the uterus in its unimpregnated state; and Mr. HUNTER's time was so much occupied by his public appointments that he had not sufficient leisure to examine them.

In the course of the last summer, I have received from Mr. LANG, by the hands of Mr. CONSIDAN, and Major NEPEAN, several preparations of the uterus in different states, and the young kanguroo at a very early period after leaving the uterus. These, on examination, appear to compose a body of evidence that elucidates several parts of the curious mode of generation of this animal, and to contain the most material anatomical facts that are necessary to direct our future inquiries.

The preparations themselves I have deposited in the collection for which they were originally intended; and am desirous to communicate the facts and observations to this learned Society, that they may prove useful to those gentlemen whose residence in that country enables them to prosecute, and complete, this interesting investigation.

The only general circumstances I have been able to collect

respecting the breeding of the kanguroo, from those who have resided in New South Wales, are the following. That they breed at all seasons; that the female has never been known to have more than a single young one at a time, and is seldom found without one. That the young remains in the false belly, or goes into it occasionally, and sucks the mother a long time after it appears capable of procuring its own food; and yet if the mother is closely pursued, in attending to her own safety, she forces the young out of the false belly, if it has arrived at a sufficient age to be covered with hair, although incapable of making its escape.

There are two male and several female kanguroos at the royal menagerie at Richmond, and two or three of the females have bred since they came there. I have visited them at different times, with a view to obtain further information upon this subject, but have been able to do little more than confirm what has been already related.

None of them have had a young one oftener than once in twelve months; and the young appears to be nine months old before it leaves off entirely sucking the mother. One of the females bred at Richmond had a young one in the false belly when only about a year and half old. The young, after it is excluded from the false belly, and another is deposited in it, continues to put in its head and suck for a month or two.

When the female is in heat, the males have no jealousy respecting one another; for a female having been covered by one of the males when the other was present, went directly and was covered by the other.

The male is retromingent; but when the penis is erect it changes its direction, and comes forwards, as in most other ani-

mals; it is of considerable length, and tapers towards the end of the glans, which is extremely small, and pointed. The testicles are contained in a very pendulous scrotum, situated upon the belly, before the penis; the scrotum is more commonly drawn up to the abdominal muscles, but at other times it hangs down several inches in length; this appears to be one of the effects of the animal's desires, at least it was so in one of the male kanguroos at the menagerie at Richmond; for when the animal was at rest the scrotum was drawn up, but when the penis was brought into the state of erection, the scrotum became extremely pendulous.

In the female, the external parts of generation are situated close to the anus, there being one common verge of the external skin to both these canals, which are only separated from one another by means of a septum of no considerable thickness. This common verge of the external skin projects above two inches beyond the bones of the pelvis, and admits of a good deal of motion.

From this structure, both in the male and female, it is evident that they copulate in the same way as most other quadrupeds.

In giving an anatomical description of the female organs of the kanguroo, I shall, with a view to avoid unnecessary detail, describe them first in their most natural, or unimpregnated state, and afterwards take notice of the changes they undergo during pregnancy, and in the time of parturition. In this description I shall be the less minute, as accurate drawings of the parts are annexed, which will explain whatever may appear to be deficient in the description.

At the external orifice of the vagina is situated the clitoris,

which when compared with the size of the other parts may be said to be large, and is covered by a præputium. A little way further on in the vagina are two orifices, which are the openings of the ducts of COOPER's glands. The vagina itself is about an inch and half in length, beyond which it is divided into two separate canals, and on the ridge which lies between them opens the meatus urinarius leading to the urinary bladder.

These two canals are extremely narrow for about a quarter of an inch in length, and their coats at this part very thick, but afterwards they become more dilated; they diverge in their course, and pass upwards for nearly four inches in length; they then bend towards each other, so as to terminate laterally in the two angles of the fundus of the uterus, of which they appear to be an uniform continuation.

The uterus itself is extremely thin and membranous in its coats, infundibular in its shape, and situated in the middle space between these canals; it is largest at its fundus, and becomes smaller and smaller towards the meatus urinarius, where it terminates; the uterus at that part in the virgin state being impervious.

The same internal membrane appears to be continued over the inner surface of the uterus and lateral canals; it is thrown into several folds, forming longitudinal projecting ridges; one of these constitutes a middle line, extending the whole length of the uterus, and dividing it into two equal parts.

The ovaria, as well as the fimbriæ, both in appearance and situation, resemble those of other quadrupeds; the fallopian tubes follow nearly the same course to the uterus, but a little way before they reach it they dilate considerably, forming an oval cavity; the coats of this part are also much thicker than

those of the rest of the canal, and they are supplied with an unusual number of blood-vessels, giving these cavities a glandular appearance. The fallopian tubes, after having formed these oval enlargements, contract again, and pass perpendicularly through the coats of the uterus at its fundus, and terminate in two projecting orifices, one on each side of the ridge formed by a fold of the internal membrane.

In the impregnated state, these parts undergo a considerable change; in one of the ovaria there is distinctly to be seen a corpus luteum; the ovaria become more vascular, as well as the oval dilatations of the fallopian tubes, which are also enlarged.

The uterus, and two lateral canals, have their cavities very much increased in size, but that of the uterus is the most enlarged: the communication between these canals and the vagina is completely cut off, by the constricted parts close to the vagina being filled with a thick inspissated mucus; and in this state of the parts there is an orifice very distinctly to be seen, close to the meatus urinarius, large enough to admit a hog's bristle, leading directly into the uterus, where in the virgin state no such passage could be observed.

The uterus and lateral canals are uniformly distended with an animal gelly, somewhat resembling the white of an egg; but the parts having been preserved in spirits during a long voyage, this substance must have lost considerably of its natural appearance.

In the cavity of the uterus I detected a substance, which appeared organized; it was enveloped in the gelatinous matter, and so small as to make it difficult to form a judgment respecting it; but when compared with the fœtus after it becomes attached to the nipple, it so exactly resembled the

back-bone with the posterior part of the skull, that it is readily recognized to be the same parts in an earlier stage of their formation.

I had an opportunity on the 22d of August, 1794, of reading these observations, and shewing the annexed drawings, to Mr. Considen, who was seven years an assistant surgeon to the general hospital in New South Wales, and who had paid much attention to this subject. During his residence in that country, he met with the uterus of the kanguroo in its enlarged state, three different times; in all of these the degree of distension was nearly the same; the gelatinous matter contained in the uterus, examined immediately after death, was of a bluish-white colour, in consistence like half-melted glue, and so extremely adhesive as to be with difficulty washed off from the fingers; the internal membrane of the uterus was very vascular, and even more so than that of the lateral canals. The oval enlargements of the fallopian tubes contained a gelly similar to that found in the uterus, but thinner in consistence. He found also the other appearances which I have already described, but in only one of them was the fœtus sufficiently advanced to be detected, and that resembled the backbone delineated in one of the annexed drawings.

Immediately after parturition, the parts are nearly brought back into their original state; the only circumstance deserving of notice is, that the opening leading directly from the uterus to the vagina, which is not met with in the virgin state, after being enlarged by the passage of the fœtus, forms a projecting orifice, and almost wholly conceals the meatus urinarius.

Were we to consider the uterus and its appendages in the

unimpregnated state, the two lateral canals would appear to be the proper vaginæ, particularly as they begin at the meatus urinarius, which is commonly placed at the entrance of the proper, or true vagina, and receive the penis in coition, the end of which is pointed to fit it for that purpose; in some species of the opossum the male has a double glans, each of them pointed, and diverging from the other, so as to enter both canals. But when we find these canals in the impregnated state forming with the uterus one general reservoir of nourishment for the fœtus, and all communication during that period between them and the vagina cut off, we are led to consider them more immediately as appendages to the uterus than the vagina.

The female kanguroo has two mammæ, and each of them has two nipples; they are not placed upon the abdominal muscles as in most quadrupeds, but are situated between two moveable bones connected with the os pubis, peculiar to this tribe of animals; and the mammæ are supported upon a pair of muscles which arise from these bones, and unite in the middle between them. The mammæ are covered anteriorly by the lining of the false belly, and the nipples project into that cavity; this covering is similar to the external skin, having a cuticle, and short hair thinly scattered over its surface, except at the root of the nipples, where there are tufts of some length, one at the basis of each.

The mammæ are supplied with blood from the epigastric arteries. The mammary branches run superficially under the false belly till they reach the mammæ. There is a strong muscle that comes down from the upper part of the abdominal

muscles, and adheres firmly to each of the mammæ; this muscle, when the young is sucking, will prevent the mamma being dragged from its natural situation.

The two bones which lie behind the mammæ deserve a particular description, as they are peculiar to the opossum tribe, and belong to the mammæ, and false belly, having no other apparent use but what is connected with the motion of these parts.

They are about two inches and an half long, are flattened, and at their broadest part measure nearly half an inch; they are attached to a projecting part of the os pubis, fitted for that purpose, just before the insertion of the recti abdominis muscles; this attachment to the pubis is by a very small surface, and admits of considerable motion; they have likewise a connection by a ligament half an inch in breadth, to the ramus of the pubis, which joins the ilium. From their base, which is united to the pubis in these different ways, they become narrower till they terminate in a blunted point. These bones have a pair of muscles inserted into their base, to bring them downwards and outwards; another pair into their blunted extremities to bring them forwards; a pair of broad flat muscles fill up the whole space between them, arising from their inner edge through its whole length; they serve as a sling to support the mammæ, and also to bring the bones towards each other.

Besides these additional bones, and the projection to which they are attached, there is another peculiarity in the structure of the pelvis of the female kanguroo; the two rami of the os ischium which join the pubis, have no notch between them as in other quadrupeds, but form a rounded convex surface of some breadth, projecting considerably forwards; the surface

itself is smooth, like those over which tendons sometimes pass; but the lateral parts are rough, and have a pair of muscles arising from them inserted into the skin of the false belly, to bring its mouth towards the pudendum.

The mode in which the young kanguroo passes from the uterus into the false belly has been matter of much speculation, and it has been even supposed that there was an internal communication between these cavities; but after the most diligent search, I think I may venture to assert that there is no such passage. This idea took its rise from there being no visible opening between the uterus and vagina in the unimpregnated state; but such an opening being very apparent, both during pregnancy and after parturition, overturns this hypothesis; for we cannot suppose that the fœtus when it has reached the vagina can pass out in any other way than through the external parts. That this is really the case, and that in this way it gets into the false belly, is highly probable for the following reasons.

The false belly has muscles to bring its mouth as near as possible to the opening of the vulva, which does not appear necessary for any other purpose than that of receiving the fœtus.

The bones belonging to the mammæ and false belly have muscles, which by their action will bring down both these parts towards the vulva, for which no other use can be assigned; and these parts are so much detached from the abdominal muscles, that this effect can be produced during their action to expel the fœtus from the uterus.

The vulva has naturally an unusual projection, and the margin of the pelvis immediately before it, is rounded and smooth,

so as to admit of its moving easily in that direction; add to this the action of opening the mouth of the false belly, will bring down the skin, and allow the external orifice of the vagina to be thrown still further out, so as to project more directly over the mouth of the false belly in which the fœtus is to be deposited.

It is to be observed, that if the parts in their natural state are fitted for such an action, they will be still more so at the period in which it is to be performed; since in all animals, at that particular time, there are changes going on to facilitate the expulsion of the young in the way most favourable for its preservation.

The size of the fœtus at the time it leaves the uterus, I believe, is not ascertained; but it has been found in the false belly attached to the nipple not more than an inch and a quarter in length, and 31 grains in weight, from a mother weighing 56 pounds. In this instance the nipple was so short a way in the mouth that it readily dropped out, we must therefore conclude that it had been very recently attached to it.

The fœtus at this period had no navel string, nor any remains of there ever having been one; it could not be said to be perfectly formed, but those parts which fit it to lay hold of the nipple were more so than the rest of the body. The mouth was a round hole, just large enough to receive the point of the nipple; the two fore-paws, when compared with the rest of the body, were large and strong, the little claws extremely distinct; while the hind legs, which are afterwards to be so very large, were both shorter and smaller than the fore ones.*

* Since writing the above, I have received from Mr. Laing, in the month of March,

When the fœtus first adheres to the nipple, the face appears to be wanting, except the round hole to receive it; and as the jaws and lips grow, they cover a greater length of the nipple, giving the mouth a better hold; the upper surface of the tongue, as that organ grows, is concave, adapting it to the nipple which lies upon it. The growth of the fœtus is distinctly seen in the annexed drawings.

From the peculiarities in the structure of the female organs of the kanguroo, it is evident they must, in their mode of generation, materially differ from other quadrupeds.

The semen of the male passes in a circuitous way through the lateral canals to the cavity of the uterus, and from the structure of the parts, can neither enter the fallopian tubes, nor readily return to the vagina.

The embryo, in its passage from the ovarium along the fallopian tube, will be enveloped in the gelly formed in the oval glandular enlargement of that canal, and in this state deposited in the uterus, where it will come in contact with the semen of the male.

This differs from other quadrupeds, but exactly coincides with all those animals whose fœtuses are detached; the semen being retained in the lower part of the oviduct, where it comes in contact with the egg when completely formed.

1795, a fœtus taken from the false belly, smaller than any that had been met with. It weighed 21 grains at the time it was taken from the false belly, and was less than an inch in length. Its fore-paws, while of this size, were equally well formed to appearance as in the fœtus above described, and double the length of the hinder ones, but the mouth had evidently less width. The nipple to which it had been attached did not accompany it.

It would seem probable, that the mouth of the fœtus is originally attached to the nipple by means of the gelatinous substance contained in the uterus.

In other quadrupeds the influence of the semen is ascertained to have reached the fallopian tube, by well attested cases of the fœtus never arriving at the uterus. In this animal such an effect is rendered difficult, and not very probable; it is therefore more natural to suppose the impregnation takes place in the same way as in the detached fœtuses of other animals.

This mode of nourishing the young resembles, in some respects, what takes place in the dog-fish, whose egg is deposited in the oviduct, and hatched there. The yelk of the egg in the bird being conveyed into the belly at the time of its being hatched, made me desirous to see if any of the gelatinous substance of the uterus was conveyed into the belly of the young kanguroo, but I could not on dissection find any such appearance; and as it is to be immediately attached to the nipple, there is no apparent necessity for such a provision.

The egg of the turtle and dog-fish, which live in water, is similar to the contents of the uterus in the kanguroo in being composed of one substance only, which renders it probable that in birds it is made up of two substances, on account of the young being longer unable to procure its own food.

If we consider the varieties which occur in the formation of different animals as so many parts of the same system, the mode of generation just described will be found, in this chain of gradations of nature, to form a link between animals whose young are nourished by means of a connection with the uterus, and those that are nourished independant of it.

EXPLANATION OF THE PLATES.

Tab. XVIII.

Fig. 1. a posterior view of the uterus, and its appendages, the rectum being removed. The parts are represented of their natural size.

a, the clitoris, inclosed in its præputium.

b b, the ducts of COOPER's glands.

c c, the internal surface of the vagina.

d, the meatus urinarius.

e e, the canals leading from the vagina to the uterus.

f f, two natural constrictions in the canals.

g g, the canals terminating in the uterus.

h h, the uterus, seen through the membrane to which the lateral canals are attached.

i i, the fallopian tubes, forming two oval swellings before they enter the uterus.

k k, the course of the fallopian tubes.

l, the ovarium of one side, slit open.

m, the other ovarium, with the fimbriæ spread over it.

n n, the ureters, passing to the bladder behind the uterus.

Fig. 2. the false belly, of its natural size in the virgin state, containing the two mammæ, each of them having two nipples, scarcely projecting above the surface. The lining of the bag has a dark-coloured cuticle, thinly covered with short hair, except at the root of the nipples, where there are tufts of some length.

Tab. XIX.

Fig. 1. and 2. represent the vagina exposed in the same manner as in the former drawing, to shew its appearance. The first is during pregnancy; and an orifice is seen close to the meatus urinarius, which leads to the uterus, and is not to be found in the virgin state. In the second, this orifice is so much enlarged as almost wholly to conceal the passage to the bladder; it puts on this appearance immediately after parturition.

Fig. 3. an anterior view of the uterus and its appendages, immediately or a short time after parturition.

a, the portion of the urinary bladder.

b b, one of the canals leading from the vagina to the uterus.

c c, the other canal, laid open.

d d, the cavity of the uterus.

e e, the openings of the fallopian tubes.

ff, a ridge made by a fold of the internal membrane.

g, the remains of a corpus luteum in the ovarium.

h, an uncommon number of blood-vessels going to the oval glandular enlargement of the fallopian tube.

i i i i, the ureters, terminating in the bladder.

Tab. XX.

Fig. 1. the fœtus of a kanguroo found in the false belly, represented of its natural size; weighing only 21 grains, and the smallest that has been ever discovered. It is probably in the earliest state; as the mouth had little if any hold of the nipple.

Fig. 2. the part of the fœtus found in the impregnated uterus.

Fig. 3. the fœtus, after having become attached to the nipple.

Fig. 4. the nipple, to show how far it had been in the mouth.

Fig. 5. the fœtus a little further advanced, and the tongue, concave on its upper surface, adapted to the nipple.

Fig. 6. the fœtus still larger, the hind legs having acquired their natural proportion to the other parts.

Tab. XXI.

A view of the pelvis of the natural size, to show the situation of the two bones belonging to the false belly.

a a, the two bones, one in its most common position, the other bent down, to show the extent of its motion.

b, the projection of the bones of the pubis, on which the two small bones move.

c, a ligament, connecting the small bone to the ramus of the os pubis.

d, a projecting rounded convex surface, over which the pudendum is brought forward, to allow of the fœtus being deposited in the false belly.

X. *On the Conversion of Animal Substances into a fatty Matter much resembling Spermaceti. By* George Smith Gibbes, *B. A. Communicated by* George Shaw, *M. D. F. R. S.*

Read March 12, 1795.

In a paper which the Royal Society have done me the honour of inserting in the last Volume of their Transactions, I related some experiments on the decomposition of animal muscle. I regret that it has not been in my power to pursue these inquiries with the attention the subject seems to demand. I beg leave, however, to present the few additional facts contained in this paper, not by any means as a full investigation of the subject, but as serving to excite the attention of those, who have more opportunities, and are better qualified, to pursue such inquiries.

I mentioned in my former paper, that the substance procured either by means of water, or the nitrous acid, appeared to me to have precisely the same external characters; but I have observed since, that there is a difference between that which I obtain from quadrupeds, and that which is procured from the human subject: the former seems not disposed to crystallize, while the latter assumes a very beautiful and regular crystalline appearance.

The matter which I procured from human muscle was melted, into which I plunged a very sensible thermometer,

which soon rose to 160°; it began congealing at 112°, and became so solid at 110° that the thermometer could not easily be taken out.

I took some of the spermaceti of the shops, and under the same circumstances I plunged the same thermometer into it. It soon rose to 170°; a pellicle was formed at the top of it when at 117°; and it became so solid at 114°, that the thermometer could not easily be taken out.

I dissolved a piece of the substance, which I had formed by means of water and the nitrous acid, in boiling spirits of wine; on cooling this mixture, a great quantity of this waxy matter was separated in the form of beautiful flakes. I could not procure large crystals, but the flakes assumed a crystalline appearance.

I put into an earthen retort some of this waxy matter, to which I added some finely powdered charcoal; on applying a pretty strong fire, a small quantity of an oily fluid came over, which concreted on cooling; after which came over a prodigious quantity of thick white vapours, which were very suffocating and offensive.

I had a copper retort made, for the purpose of trying some experiments on this matter. I put a small quantity into it, and placed it on a common fire; there came over first a limpid fluid like water, without much smell; on the addition of more heat, there came over an oily fluid, which soon coagulated, of a firmer consistence than when put in, and coloured of a beautiful green by the copper; this last circumstance proves that it contained no ammonia.

Having procured some very pure quicksilver, I took a glass, which contained about 10 pounds of that fluid, with which I

filled it; I inverted it in a bason, which contained the same fluid; I introduced a small piece of lean meat, and also a small quantity of water; at the end of about six weeks, so great a quantity of gas was disengaged as nearly to occupy the whole of the vessel; the meat had assumed a white appearance.

Since I mentioned my former experiments on the cow, which I had submitted to the action of running water, I have observed a few facts relating to the changes which took place. This cow was placed in a situation where the water could come twice every day, as before described; over it some loose earth was thrown: after it had remained some time in this place, I used frequently to push a stick through this earth to the cow; every time this was done there came up a prodigious quantity of air, after I had suffered it to remain quiet for a short time. Since I put this cow in this situation, I have had two horses and another cow placed under the same circumstances; in all of them this disengagement of air takes place; this air is extremely offensive.

In the former cow the whole muscular part seemed changed; and from the substance formed I have procured a very large quantity of a waxy substance by means of the nitrous acid. Though the nitrous acid takes off the greatest part of the fœtor from the substance thus formed, yet it gives it a yellow colour which is with difficulty removed, and a peculiar smell, evidently similar to the smell of the acid employed, which mere washing and the addition of alkalies will not entirely remove.

My father, who has been indefatigable in his attempts to whiten this substance, finds that the following process will make it very pure, and very beautiful, though not so white as

the spermaceti of the shops. The cow, which had lain in the water for a year and an half, was taken up, and we found that the whole muscular part was perfectly changed into a white matter; this was broken into small pieces, and was exposed to the action of the sun and air for a considerable length of time. By these means it lost a great deal of its smell, and seemed to acquire a firmer consistence. The appearance of this substance was somewhat singular; for on breaking it, we found little filaments running in every direction, exactly similar to the cellular substance between the muscular fibres. These pieces were then beaten to a fine powder, and on this powder was poured some diluted nitrous acid; after the acid had been on it for about an hour, a froth was formed at the top; the acid was then poured off, and the substance was repeatedly washed; it was then melted in hot water, and when it concreted it was of a very beautiful straw-colour, without the least offensive smell, on the contrary, it had the agreeable smell of the best spermaceti. May not this substance be applied as an article of commerce? Great quantities of it may be obtained. It burns with a fine flame; and dead animals, which at present are of little or no use, may be changed into it. I am very sorry that it has not been in my power to ascertain the precise quantity which may be obtained from a given quantity of flesh; but from what I have obtained, I can say that it would be very considerable. The running water carries off a great deal of it, but that might be obviated by the addition of strainers. Moreover, that which is carried off by the water is the purest, for I always take care to get as much as possible of it, because I find it gives me less trouble in purifying it. The water over the animals, and for some distance round them, is covered with

a very beautiful pellicle, which is white in general; sometimes it refracts the sun's rays, producing the prismatic colours.

Fish may be also changed; and I recollect having seen in some old author, whose name I cannot recollect, a passage in which he mentions a circumstance where something of this kind happened in a whale. He says, that after this fish has been putrifying on the shore some time, the people have a secret by which they can procure and purify lumps, which they find to be similar to the spermaceti which they get in the usual way.

I have heard from many people, observations which they had made where this substance had been formed, and which they could not account for; but as the circumstances were the same as those beforementioned, I shall forbear giving additional trouble.

On seeing a body opened some time ago, where there was a great collection of water in the cavity of the thorax, I observed that the surface of the lungs was covered with a whitish crust. I remarked to a friend, that I thought this crust was owing to some combinations which had taken place between the lungs or pleura and the serous fluid effused, similar to what I had observed between flesh and water; or that the serous fluid had acted on the coagulable matter, and had produced a similar change.

Dr. CLEGHORN mentions a circumstance, which in some measure seems to agree with the observation then made. As the fact is a curious one, I shall subjoin the following extract. He is speaking of abscesses formed in the lungs. "These abscesses had sometimes emptied themselves into the cavity of "the thorax, so that the lungs floated in purulent serum, their

"external membrane, and likewise the pleura, being greatly "thickened, and converted as it were into a white crust, like "melted tallow grown cold." In a note he says, "I am now "doubtful if this crust was the pleura and external coat of the "lungs, changed from a natural state by soaking in a puru- "lent fluid, and if it was not altogether a preternatural sub- "stance, formed by fluids deposited on those membranes, and "compacted together by the motion of the lungs."

Much has been said by many authors on the subject of secretion. It was at one time supposed that it depended on some peculiar property of the living principle; and it was thought impossible to form any secretion but through the medium of secreting organs. M. FOURCROY has, however, contradicted this by the experiments where he forms bile.

Spermaceti is an animal substance, secreted in a particular species of whale, and the substance which is formed in the foregoing experiments, as far as I can judge, agrees with it in every particular.

M. FOURCROY says, that M. POULLETIER DE LA SALLE found a crystallized inflammable substance similar to spermaceti in biliary calculi.

May not the suety matter in steatomatous tumours arise from something of this kind?

By attending to the various secretions of the body, by examining their composition in the healthy and morbid states of the system, may we not expect to derive great advantage, particularly when accurate experiments are applied towards the relief of disease?

Some excuse may perhaps seem necessary for the little attention which has been paid to the accurate results in the

different experiments; particularly so, as the analysis of every part of the animal body, except the bones, is at present so incomplete; but I hope that the time necessary for my medical pursuits, and the want of a complete chemical apparatus, will not render the simple facts I have here related less useful.

I have not attempted to account for the various phænomena which appear in the experiments, because the facts seem too few to admit of any general conclusion.

If the above experiments should appear to the Society worthy of their attention, the application of my former experiments, and the results of some which I hope to make, on some animals that are placed under different circumstances favourable to their decomposition, shall be the basis of a future paper.

XI. *Observations on the Influence, which incites the Muscles of Animals to contract in Mr.* Galvani's *Experiments. By* William Charles Wells, *M. D. F. R. S.*

Read March 19, 1795.

Mr. Volta, in his letters to Mr. Cavallo, which have been read to this Society, not only has shewn that the conclusions, which Mr. Galvani drew from his experiments on the application of metals to the nerves and muscles of animals, are in various respects erroneous, but has also made known several important facts, in addition to those which had been discovered by that author. As he appears, however, from these letters, to have fallen into some mistakes himself, and has certainly not exhausted the subject which he has treated in them, I shall venture to communicate to this learned body a few observations I have made respecting it, which may contribute both to correct his errors, and to increase our knowledge of the cause of those motions, which have been attributed by Mr. Galvani and others to an animal electricity. These observations will be so arranged, as to furnish answers, more or less satisfactory, to the following questions: Does the incitement of the influence which, in Mr. Galvani's experiments, occasions the muscles of animals to contract, either wholly, or in part, depend upon any peculiar property of living bodies? What are the conditions necessary for the excitement of this influence? Is it electrical?

When a muscle contracts upon a connection being formed, by means of one or more metals, between its external surface and the nerve which penetrates it, Mr. GALVANI contends that, previously to this effect, the inner and outer parts of the muscle contain different quantities of the electric fluid; that the nerve is consequently in the same state, with respect to that fluid, as the internal substance of the muscle; and that upon the application of one or more metals between its outer surface and the nerve, an electrical discharge takes place, which is the cause of the contraction of the muscle. In short, he supposes a complete similarity to exist between a muscle, in a proper condition to exhibit this appearance, and a charged Leyden phial; the nerve of the former answering, as far as his experiments are concerned, the same purpose as the wire, which is connected with the internal surface of the latter.

Now, if this were just, such a muscle ought to contract, whenever a communication is formed between its internal surface and the nerve, by means of *any* conductor of electricity; and accordingly Mr. VOLTA, who to a certain extent adopts Mr. GALVANI's theory, asserts this to be the case, as often as the experiment is made upon an animal which has been newly killed. But I am inclined to believe that he rests this assertion upon some general principle, which he thinks established, and not upon particular facts; for he gives none in proof of it, and I have often held a nerve of an animal newly killed in one hand, while with the other I touched the muscle to which the nerve belonged, but never saw contractions by this means excited. I have also frequently taken hold of a nerve of an animal, which was recently killed, with a non-conductor of electricity, and have in this way applied its loose

end to the external surface of the muscle which it entered, without ever observing motion to follow. I think, therefore, I am entitled to conclude, not only that the theory advanced by Mr. GALVANI, respecting the cause of the muscular motions in his experiments, is erroneous; but also, that the influence, whatever its nature may be, by which they are excited, does not exist in a disengaged state in the muscles and nerves, previously to the application of metals. Should it be urged against this conclusion, that, since metals are much better conductors of electricity than moist substances, the charge of a muscle may be too weak to force its way through the latter, though it may be able to pass along the former; my answer is, that, in all Mr. GALVANI's experiments, the nerve makes a part of the connecting medium between the two surfaces of the muscle, and that the power of no compound conductor can be greater than that of the worst conducting substance, which constitutes a part of it.

It may be said, however, that, although there is no proof that any influence naturally resides in the nerves or muscles, capable of producing the effects mentioned by Mr. GALVANI, these substances may still, by some power independent of the properties they possess in common with dead matter, *contribute* to the excitement of the influence, which is so well known to exist in them, after a certain application of metals. Before I enter upon the discussion of this supposition, I must observe, that there are two cases of such an application of metals; the first is, when we employ only one metal; the second, when we employ two or more. With respect to the first case, a late author, Dr. FOWLER, who seems to have made many experiments relative to this point, positively asserts, that he never saw a fair in-

stance of motion being produced by the mere application of a single metal to a muscle and its nerve. I shall, therefore, defer treating this case, till I speak of the conditions which are necessary for the excitement of the influence. Nor will the present subject suffer from this delay; for if it be shewn, as I expect it will, that, when two or more metals are used, the muscle and its nerve do not furnish any thing but what every other moist substance is equally capable of doing, it will, I think, be readily granted, that they can give nothing more, when only one metal is applied to them.

In regard to the second case, Mr. VOLTA has affirmed, or has said at least, what I regard as equivalent to affirming, that, when two metals are employed, the influence in question is excited by their action upon the mere moisture of the parts which they touch. The proofs, however, of this assertion were reserved for some future communication. But as more than two years have now elapsed since they were promised, and none have been given to this Society, or have appeared, as far as I can learn, in any other way, I hope I shall not be thought precipitate, if, at this distance of time, I offer one of the same point, which seems to me both plain and decisive.

It is known, that, if a muscle and its nerve be covered with two pieces of the same metal, no motion will take place upon connecting those pieces, by means of one or more different metals. After making this experiment one day, I accidentally applied the metal I had used as the connector, and which I still held in one hand, to the coating of the muscle only, while with the other hand I touched the similar coating of the nerve, and was surprised to find that the muscle was immediately thrown into contraction. Having produced motions

in this way sufficiently often to place the fact beyond doubt, I next began to consider its relations to other facts formerly known. I very soon perceived, that the immediate exciting cause of these motions could not be derived from the action of the metals upon the muscle and nerve, to which they were applied; otherwise it must have been admitted, that my body and a metal formed together a better conductor of the exciting influence than a metal alone, the contrary of which I had known, from many experiments, to be the case. The only source, therefore, to which it could possibly be referred, was the action of the metals upon my own body. It then occurred to me, that a proper opportunity now offered itself of determining, whether animals contribute to the production of this influence by means of any other property than their moisture. With this view, I employed various moist substances, in which there could be no suspicion of life, to constitute, with one or more metals, different from that of the coatings of the muscle and nerve, a connecting medium between those coatings, and found that they produced the same effect as my body. A single drop of water was even sufficient for this purpose; though, in general, the greater the quantity of the moisture which was used, the more readily and powerfully were contractions of the muscle excited. But, if the mutual operation of metals and moisture be fully adequate to the excitement of an influence capable of occasioning muscles to contract, it follows, as an immediate consequence, that animals act by their moisture alone in giving origin to the same influence in Mr. GALVANI's experiments, unless we are to admit more causes of an effect than what are sufficient for its production.

Before I dismiss this part of my subject I may mention, that, being in possession of a method to determine what substances are capable, along with metals, of exciting the influence, I made several experiments for the purpose of ascertaining this point. I found, in consequence, that all fluid bodies, except mercury, that are good conductors of electricity, all those at least which I tried, can with the aid of metals produce it. The bodies I tried, beside water, were alcohol, vinegar, and the mineral acids; the last both in their concentrated states, and when diluted with various portions of water. Alcohol, however, operated feebly. On the other hand, no fluid, which is a non-conductor of electricity, would assist in its production: those upon which the experiment was made were the fat and essential oils. Ether, from its similarity to alcohol, I expected would also have concurred in the excitement of the influence, but it did not; neither would it conduct the influence when excited by any other means. I may remark, however, that the ether I employed had been prepared with great care; other ether, therefore, less accurately made, may possibly be found to contribute to the excitement of the influence, either from the undecomposed alcohol, or naked acid, it may contain.

Having thus given an answer to the first question, I proceed to the discussion of the second.

It has hitherto been maintained by every author, whose works I have read upon the subject of Mr. GALVANI's experiments, and by every person with whom I have conversed respecting it, that metals are the only substances capable, by their application to parts of animals, of exciting the influence, which in those experiments occasions the muscles to

contract. But it appears rather extraordinary, that none of those, who contend for the identity of this influence and the electric fluid, have ever suspected, that the only very good *dry* conductor of the latter which we know, beside the metals, possesses like them the property of exciting the former. I confess, however, that it was not this consideration, but accident, which led me to discover that charcoal is endowed with this property, and in such a degree that, along with zinc, it excites at least as strongly as gold with zinc, the most powerful combination, I believe, which can in this way be formed of the metals. But to prevent disappointments I must mention, that all charcoal is not equally fit for this purpose, and that long keeping seems to diminish its power.

It being shewn that charcoal is also to be ranked among the exciters of this influence, I shall now speak of the circumstances, in which both it and the metals must be placed, to fit them for the exercise of their power. With respect to metals, Mr. VOLTA maintains, that to this end it is only necessary, that two different species be applied to any other body which is a good conductor of electricity, and that a communication be established between the two metallic coatings. But charcoal is a much better conductor of electricity than water, and yet metals in contact with it alone will not excite. Again, Mr. VOLTA says, that the simple application of two metals to two parts of an animal disturbs the equilibrium of the electric fluid, and disposes it to pass from one of the parts to the other, which passage actually takes place, as soon as a conductor is applied between the metals. But what should prevent the passage of the fluid *before* the application of a new conductor, since the metals were already connected by means

of the moisture of the animal? Further, a consequence of this opinion is, that, if the under surfaces of two different metals be placed in moisture, and their upper surfaces be afterwards connected by means of a nerve, still attached to its muscle, contractions ought then to be produced; since the whole quantity of the electric fluid necessary to restore the equilibrium, which has been disturbed by the action of the metals, must pass through the nerve. This experiment I have made, and as I did not find the muscle to contract, I must hold Mr. VOLTA's opinion on this point to be likewise ill founded. The fact is, that as far as the contraction of muscles is a test, whether the influence exists or not, and we have no other, it is never excited, when two metals, or one metal and charcoal are necessary for this purpose, unless these substances touch each other, and are also in contact with some of the fluids formerly mentioned.

But there is still another requisite for the excitement of the influence, which is a communication, by means of some good conductor of electricity, between the two quantities of fluid, to which the dry exciters are applied, beside that which takes place between the same quantities of fluid, when the dry exciters are brought into contact with each other. As from this last circumstance, a complete circle of connection is formed among the different substances employed, it has been imagined by many, that the individual quantity of the influence excited goes the whole round, each time contraction is produced. There is an experiment however, first, I believe, made by Dr. FOWLER, which appears to contradict this opinion. He brought two different metals into contact with each other in water, at the distance of about an inch from the divided end of a nerve,

placed in the same water, and found that the muscles, which depended upon it, were from this procedure thrown into contractions. Now, in this experiment, there was surely room enough for the influence to pass through both metals, and the moisture immediately touching them, without going near to the nerve. I think it, therefore, probable, that motions are in no case produced by any thing passing from the dry exciters through the muscles and nerve, but that they are occasioned by some influence, naturally contained in those bodies as moist substances, being suddenly put in motion when the two dry exciters are made to touch both them, and each other; in like manner as persons, it is said, have been killed by the motion of their proper quantity of the electric fluid. But to return from conjecture to facts, I shall now examine, whether it be always necessary to employ two dry exciters, that is, two metals, or one metal and charcoal, in order to occasion contractions.

Gold and zinc, the first the most perfect of the metals, the other an imperfect one, operate together very powerfully in producing contractions; while gold, and the next most perfect metal, silver, operate very feebly. It would seem, therefore, that the more similar the metals are, which are thus used, the less is the power arising from their combination. Two pieces of the same metal, but with different portions of alloy, are still more feeble than gold and silver; and the power of such pieces becomes less and less, in proportion as they approach each other in point of purity. From these facts it has been inferred, that, if any two pieces of the same metal were to possess precisely the same degree of purity, they would if used together be entirely inert, in regard to the excitement of muscular contrac-

tions; in confirmation of which, many persons have asserted, that they have never observed muscles to move from the employment of two such pieces of metal, or of one piece of metal having the same fineness through its whole extent. Others, however, upon the authority of their observations, have maintained the contrary; and to the testimony of these I must add my own, as I have frequently seen muscular motions produced not only by a single metal, but likewise by charcoal alone. Nor will credit be denied me on this head, after I have pointed out certain practices, by which any one of those substances may at pleasure be made to produce contractions. The most proper way of mentioning these practices will, perhaps, be to relate in what manner they came to my knowledge.

I one day placed a piece of silver, and another of tin-foil, at a small distance from each other upon the crural nerve of a frog, and then applied a bent silver probe between them, with the view of ascertaining, whether contractions would arise, agreeably to Mr. VOLTA's declaration, from the influence passing through a portion of the nerve without entering the muscles. Having finished this experiment, I immediately after applied the same probe between the silver coating of the nerve and the naked muscles, and was surprised to see these contract. A second and third application were followed by the same effects, but further applications were of no avail. It then occurred to me that motions might re-appear, if I again touched the two coatings with the probe, and the event proved the conjecture to have been fortunate; for after every application of the probe to the two coatings, contractions were several times excited by it. The fact being thus established, that under certain circumstances contractions could be produced by silver alone,

it next became a subject of inquiry, whether this was owing to any disposition of the muscles and nerve, which had been induced upon them by Mr. VOLTA's experiment, or whether, the condition of the muscles and nerve being unaltered by that experiment, the silver had gained some new property by coming into contact with the tin-foil. The point in doubt was soon determined, by applying the probe to a piece of tin-foil, which had no connection with any part of the animal; for, when this was done, it was again enabled to produce contractions. As these experiments, however, frequently did not succeed when made upon other frogs, I afterwards varied the metals, and found in consequence, that zinc, particularly if moistened, communicated an exciting power pretty constantly to silver, gold, and iron. If any of these metals were slightly rubbed on the zinc, they almost always acquired such a power.

It will, perhaps, be thought from the last-mentioned circumstance, that, in every instance of motion being in this way produced, it was in truth owing to some part of one of the metals having been abraded by the other; so that, under the appearance of one metal, two were in reality applied. But it can scarcely be supposed, that, from touching the polished surface of tin-foil in the gentlest manner with the smooth round end of a silver probe, any part of the former metal was carried away by the latter; and even when friction was used, as the zinc was much harder than the gold and silver, it is not probable that it was in the least abraded by them. Besides, moisture, as I have already said, increases this effect of friction, though it lessens friction itself.

The most powerful argument, however, in favour of my

opinion, is another fact I discovered in pursuing this subject; which is, that an exciting power may be given to a metal by rubbing it on many substances beside another metal, such as silk, woollen, leather, fish-skin, the palm of the human hand, sealing-wax, marble, and wood. Other substances will, doubtless, be hereafter added to this list.

As the metals while they were rubbed were held in my hand, which, from the dryness of its scarf-skin, might have afforded some resistance to the passage of small quantities of the electric fluid; and as the substances, upon which the friction was made, were either electrics, or imperfect conductors of electricity; I once thought it possible, that the metal subjected to the friction had acquired by means of it an electrical charge, which, though very slight, was still sufficient to act as a stimulus upon the nerves to which it was communicated. But that this was not the case was afterwards made evident, by the following experiments and considerations.

1. A metal, rendered capable by friction of exciting contractions, produced no change upon Mr. BENNET's gold-leaf electrometer.

2. The interposition of moisture does not, in any instance I know of, increase the effect of friction in exciting the electric fluid. In some instances it certainly lessens this effect. But moistened substances, when rubbed by a metal, communicate to it the capacity of producing contractions, much more readily than the same substances do when dry.

3. If my hand, from being an imperfect conductor, had occasioned an accumulation of electricity in the metal which was rubbed, a greater effect of the same kind ought certainly

to have been produced by insulating the metal completely; which is contrary to fact.

4. I placed a limb of a frog, properly prepared, upon the floor of my chamber; if a severe frost had not prevailed when I made this experiment, I should have laid it upon the moistened surface of the earth. I then raised from the muscles, by means of an electric, the loose end of the nerve, and touched it with the rubbed part of a piece of metal; but no contractions followed. To be convinced that this was not owing to any want of virtue in the metal, I kept the same part of it still in contact with the nerve, while I applied another part to the muscles; immediately upon which contractions were excited.

5. Admitting now the limb of an animal to be in such an experiment completely insulated, and that the metal actually becomes electrical from the friction it undergoes, surely a very few applications can only be required to place them both in the same state with respect to the electric fluid; and when this happens, all motions depending on the transflux of that fluid must necessarily cease. I have found, however, that a piece of metal which has been rubbed will excite contractions, after it has been *many* times applied to the limb. In one instance, vigorous contractions were occasioned by the 200th application; and if I had chosen to push the experiment further, I might certainly have produced many more. I may mention also, as connected with this fact, that I have frequently observed a piece of metal to excite motions, an entire day after it had been rubbed.

What I have said will, probably, be thought more than sufficient to prove, that metals, after being rubbed, do not produce

muscular contractions by means of any disengaged electricity they contain. If my opinion were now asked, respecting the mode in which friction communicates such a power to them, I should say, that the part which has been rubbed is so far altered, in some condition or property, as to be affected differently, by the fluid exciters, from a part which has not been rubbed; in short, that the rubbed part becomes, as it were, a different metal. There are two facts, beside those already mentioned, which support this conjecture. The first is, that when I have endeavoured to give an equal degree of friction to the two parts of the metal which I applied to the muscle and its nerve, little or no motion was excited by it; so that it is reasonable to suppose, that, if precisely the same degree of friction were given to both the parts, no contractions would ever be produced by them, when used in this way. The second is, that, although only one part of the metal be rubbed, still, if both the muscle and nerve be coated with some other metal, the application of the rubbed metal between these similar coatings will not be followed by motions; which, however, will immediately be produced, by touching the naked muscle and nerve with the same piece of metal. But, whether any part of my reasoning upon this head be admitted as just or not, it must yet be granted, as I think I cannot be mistaken respecting the facts which have been mentioned, that very slight accidents may give the power of exciting contractions to a single metal, which had it not before; and that we may hence easily account for the discordant testimonies of authors upon this point.

Hitherto I have spoken only of the effects of friction upon metals. But to conclude this part of my subject, I must now

remark, that charcoal, though from its friability not very fit for the experiment, may yet be rendered capable by the same means of producing contractions, without the assistance of any of the metals.

My next and last object is to inquire, whether the influence, which in all these experiments immediately excites the muscles to act, be electrical or not.

The points of difference between any two species of natural bodies, even those which, from the similarity of some of their most obvious qualities, have once been thought the same, are found, upon accurate examination, greatly to exceed in number those of their agreement. When, therefore, two substances are known to have many properties in common, while their differences are few, and none of these absolutely contradict such a conclusion, we infer with considerable confidence, that they are the same, though we may not be immediately able to explain why their resemblance is not complete. After Mr. WALSH, for instance, had discovered, that the influence of the torpedo was transmitted by all the various bodies which are good conductors of the electric fluid, philosophers made little hesitation in admitting them to be one and the same substance, though some of their apparent differences could not then be accounted for. In like manner, the inquirers into the nature of the influence, the effects of which are so evident in Mr. GALVANI'S experiments, have very generally, and in my opinion justly, allowed it to be electrical, on the ground that its conductors and those of electricity are altogether the same. To this, however, an objection has been made by Dr. FOWLER, which, if well founded, would certainly prove them to be different substances; for he has asserted that charcoal, which is so

good a conductor of electricity, refuses to transmit the influence, upon which the motions in Mr. GALVANI's experiments depend. In reply I shall only say, that Dr. FOWLER must have been unfortunate with respect to the charcoal he employed; since all the pieces I ever tried, and I have tried many, were found to conduct this influence.

Other arguments have likewise been urged against the identity of the two influences; all of which, however, excepting one, I shall decline discussing, as they either are of little importance, or have not been stated with sufficient precision. The objection I mean is, that in none of the experiments with animals, prepared after the manner of Mr. GALVANI, are those appearances of attraction and repulsion to be observed, which are held to be the tests of the presence of electricity. My answer to it is, that no such appearances can occur in Mr. GALVANI's experiments, consistently with the known requisites for their success, and the established laws of electricity. For, as it has been proved that there is naturally no disengaged electric fluid in the nerves and muscles of animals, I except the torpedo and a few others, no signs of attraction and repulsion can be looked for in those substances, before the application of metals or charcoal; and after these have been applied, the equilibrium of the influence, agreeably to what has been already shewn, is never disturbed, unless means for its restoration be at the same time afforded. Neither then ought signs of attraction and repulsion to be in this case presented, on the supposition that the influence is electrical; since it is necessary for the exhibition of such appearances, that bodies, after becoming electrical, should remain so during some sensible portion of time: it being well known, for example, that

the passage of the charge of a Leyden phial, from one of its surfaces to the other, does not affect the most delicate electrometer, suspended from a wire or other substance, which forms the communication between them.

Such are the observations I mean at present to submit to the consideration of this Society, respecting the influence which incites the muscles of animals to contract, in Mr. GALVANI's experiments.

XII. *Observations on the Structure of the Eyes of Birds. By Mr.* Pierce Smith, *Student of Physic. Communicated by* George Pearson, *M. D. F. R. S.*

Read March 26, 1795.

While examining the eyes of birds, I observed in them a singular structure, which I believe has not been hitherto noticed; and though not the object I had in view in the examination, it will perhaps elucidate several remarkable circumstances in the natural history of these animals, and may ultimately be applied to the eyes of other animals, and add one additional discovery to those already made on this beautifully constructed organ.

In March, 1792, I observed, while dissecting the eyes of birds, an irregular appearance of the sclerotica, in that part of it which immediately surrounds the cornea, and which in them is generally flat. On a more minute examination, it appeared to be scales lying over each other, and which appeared capable of motion on each other. These appearances I shewed to Dr. Fowler, of London, and likewise to Mr. Thomson, surgeon, Edinburgh. In June, this paper was copied out at my request, by Mr. Irving, who resided in the same house with me. On investigating this singular structure, the scales were found to be of bony hardness, at least much more so than any other part of the sclerotica. On the inside of the sclerotic coat of the eye there was no appearance of these

scales, that part of it being similar to the rest of the sclerotica. Tendinous fibres were detected spreading over the scales, and terminating at last in forming the four recti muscles belonging to the eye, so that, upon the contraction of these muscles, motion of the scales would be produced. This imbricated appearance of part of the sclerotica, and the detection of the tendinous fibres spreading over scales terminating at last in the four recti muscles, led me to consider the use of this structure, what would be the effect of motion of the scales upon the vision of birds, and how far this can be applied to other animals.

It is a fact so well known to persons acquainted with optics that it is almost unnecessary to mention it, that the rays of light, passing through a lens, will be refracted to a point or focus beyond the lens, and this focus will be less distant in proportion as the lens approaches to a sphere in shape. Now this principle is very naturally applied to the explanation of the use of this apparatus. These scales lying each partly over the next, so as to allow of motion, will, on the contraction of the recti muscles inserted into, and covering them, move over each other, and thus the circle of the sclerotica will be diminished, and of course the cornea, which is immediately within the circle made by these scales, will be pressed forwards, or in other words rendered more convex, and thus the focus of the eye becomes altered, its axis being elongated. This construction, and consequent convexity of the cornea, must render small objects near the animal very distinct.

On these muscles relaxing, the elasticity of the sclerotic coat will restore the cornea to its original flatness; it thus becomes fitted for viewing objects placed at a greater distance

from the eye, and this will be in proportion to the degree of relaxation.

There seems to exist in nature an œconomy of motion, to prevent fatigue, and exhaustion of the animal powers, by continued voluntary muscular action. If two opposite actions of the same frequency occur in two muscles, the one being antagonist to the other, the action of one ceasing, the action of the other must take place previously to farther motion of the part; for instance, on the biceps flexor of the arm acting, the arm will be bent, but on discontinuing its action the arm will remain in the same state, unless it was straightened by the action of the biceps extensor, its antagonist: but where one action in a part is required to take place almost constantly, and the opposite action but seldom, to save the animal from fatigue, necessarily induced by muscular contraction, she gives an elastic ligament, which from its elasticity may be said to be in continual action, without exhausting the animal. Thus when the opposite action which is of less frequent occurrence is required, it is performed by overcoming the resistance, or elasticity of this elastic ligament, which on the muscle giving over its action again, resumes its former state. The elastic cartilages of the ribs, performing in some degree the functions of a muscle, are of use in respiration; likewise the elastic ligaments which support the claws of all the feline genus, keeping them from friction against the ground. These claws, at the volition of the animal, by muscles appropriated for that purpose, are brought into action, or extended. From the abovementioned structure, the same thing appears to take place in the eyes of animals. When an animal is desirous of seeing minute objects, the recti muscles act, and thus, by

rendering the eye more convex, enlarge the angle under which the object is seen. How necessary is this structure to these animals in particular; for without it a bird would be continually exposed to have its head dashed against a tree when flying in a thick forest, its motions being too rapid for the common structure of the eye. The eagle, when soaring high in the air, observes small objects on the earth below him, inconceivable to us, and darts upon them instantaneously. Here we must allow that there must be an extraordinary alteration in the focus in this eye, in almost an instant of time. How could this be performed unless the animal had this apparatus? The eyes of quadrupeds, as I shall afterwards shew, can perform this alteration, though not in the same degree, as it is not necessary, their modes of life being different. A swallow sailing through the air pursues a gnat or small fly to almost certain destruction. This apparatus is very distinct in all these birds. Wherever we find the subsistence or safety of an animal intrusted to, or depending more particularly upon one sense than the rest, we are sure to find that sense proportionably perfect; as in quadrupeds the organ of smelling is remarkably perfect, and leads them to their prey, so the eyes of birds are proportionably perfect, being the means not only of their support, but from them they receive the first intimation of approaching danger.

The eyes of birds, like those of other animals, consist of three coats, the sclerotica, choroides, and retina. The human eye, as well as those of quadrupeds, is nearly spherical; in birds the sphere is more oblate, the sclerotica as it approaches the cornea becoming suddenly flat. The cornea, though small when compared with the size of the whole eye, is more convex,

as it forms the segment of a smaller circle, added to the larger formed by the sclerotica. The reason or advantage of this flatness is not very evident. It prevents them, perhaps, from projecting so far as to expose them to danger from the trees and grass, amongst which these animals live.

As no description, however accurate, can give an exact idea of the structure of any part of the animal body, I have caused small sketches to be made, explaining all the different circumstances that I have mentioned in this paper.

After having examined the eyes of birds, and seeing this curious apparatus, I was next led to the examination of the eyes of quadrupeds, that I might see in what manner they resembled the eyes of birds, and if I could account for their being able to accommodate their eyes to objects at different distances.

This was a subject I found involved in much difficulty, as the eyes of quadrupeds appeared on examination not to have these imbricated scales, which are so obvious in birds; but all this difficulty vanished on taking hold of one of the four recti muscles of the eye of a sheep; and by tearing and dissecting, I found that it terminated in, and with the other parts composed, the cornea; so that on the first volition of the mind, the recti muscles on contracting will have the power of fixing the eye, and keeping it steady, and at the same time by contracting more or less, will adapt the focus of the eye to the distance of the object, but in a less degree than in birds. On these muscles giving over acting, the eye will be restored to its former state by the elasticity of the sclerotic coat.

From a knowledge of these circumstances, we may from

rational principles explain, why people by being long accustomed to view small objects obtain in time a sort of microscopic power, if it may be so called; that is, the muscles which contract the cornea will by custom increase their power of action, and grow stronger, like the other muscles of the body. Other phænomena of vision on these principles may be explained.

EXPLANATION OF THE PLATE (Tab. XXII.)

Fig. 1. represents the eye of a buzzard, blown up and dried, the lesser circle of the cornea suddenly rising above the sclerotic coats.

Fig. 3. is a representation of the imbricated or loricated appearance of the scales which cover part of the sclerotic coat of the eye, divested of its muscles.

Fig. 4. shews that the scaly appearance is weaker in some birds than in others, according to their different modes of life, more so in the turkey than in the buzzard, (see fig. 3.) representing likewise one of the recti muscles attached to the scales.

Fig. 5. the inside view of these scales in the eye of a turkey, the internal coat of the cornea being torn up, or separated from the external.

Fig. 6. the four recti muscles in the eye of the sheep, dissected so as to shew their fibres inserted into, and going to form, the outer coat of the cornea.

Fig. 7. the four recti muscles of the eye of the turkey,

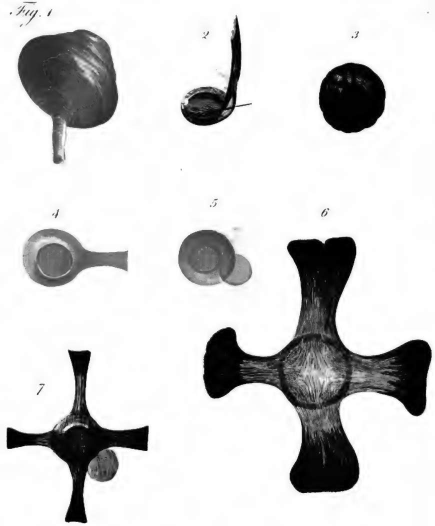
Fig. 1
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which are partly inserted into and running to form part of the outer coat of the cornea.

Fig. 2. one of the recti muscles, dissected in such a manner as to shew that a part of it is inserted into, and the rest of the muscle going to form, the outer coat of the cornea.

XIII. *Observations on the best Methods of producing artificial Cold. By Mr.* Richard Walker. *Communicated by* Martin Wall, *M. D. F. R. S.*

Read May 14, 1795.

HAVING already investigated the *means* of producing artificial cold, and at the conclusion of my last paper (on the congelation of quicksilver) dismissed *that part* of the subject, the best method of making use of those means naturally becomes a desideratum; to that therefore I have lately given my attention, and flatter myself that the following observations may be considered as an useful appendix to my former papers. The freezing point of quicksilver being now as determined a point on the scale of a thermometer, *viz.* — 39°, as the freezing point of water; and as this metal, exhibited in its solid state, affords an interesting as well as curious phænomenon; I shall apply what I have to say principally to that object.

Frequent occasions having occurred to me of observing the superiority of snow, in experiments of this kind, to salts, even in their fittest state, that is, fresh crystallized, and reduced to very fine powder, I resolved upon adopting a kind of artificial snow.

The first method which naturally presented itself, was by condensing steam into hoar-frost; this answered the purpose, as might be expected, exceedingly well; but the difficulty and

expence of materials in collecting a sufficient quantity, determined me to relinquish this mode for another, by which I can easily and expeditiously procure ice in the fittest form for experiments of this kind; the method I mean, is by first freezing water in a tube, and afterwards grinding it into very fine powder. Thus possessed of the power of making ice, and afterwards reducing it to a kind of snow, the congelation of quicksilver becomes a very easy and certain process; for by the use of a very simple apparatus (Tab. XXIII. fig. 1.) quicksilver may be frozen perfectly solid, in a few minutes, wherever the temperature of the air does not exceed 85°, thus: one ounce of nitrous acid is to be poured into the tube *b* of the vessel, observing not to wet the side of the tube above with it; a circular piece of writing paper of a proper size is to be placed over the acid, resting upon the shoulder of the tube, and the paper brushed over with some melted white wax; thus prepared, the vessel is to be inverted, and filled with a mixture of diluted nitrous acid, phosphorated soda, and nitrous ammoniac, in proper proportions for this * temperature, and tied over securely, first with waxed paper, and upon that a wet bladder.

The vessel being then turned upright, and placed in a shallow vessel, *viz.* a saucer or plate, an ounce and a half of rain or distilled water is to be poured into the tube, which is to be covered with a stopper or cork, and, as soon as frozen solid, ground to very fine powder, an assistant holding it firmly and steadily the while; observing occasionally to work the instrument in different directions up and down, that no lumps

* I have, by a very accurate preparation of this mixture, sunk a thermometer from 85° (temperature of the vessel and materials) to + 2°.

may be formed. When the whole of the ice is thus reduced to powder, and the lumps, if any, broken, the frigorific mixture is to be let out quickly, by cutting or untying the string, and removing the bladder, &c. which confines it; a communication made, by forcing a rod of glass or wood through the partition; and the whole mixed expeditiously together.

In this climate, a mixture much less expensive will be sufficient, *viz.* that composed of diluted nitrous acid, GLAUBER's salt, sal ammoniac, and nitre; a mixture of this kind sinking a thermometer in the warmest weather to near 0°. At the temperature of 70°, or a little higher, the quantity of diluted nitrous acid may be about one-fourth less than is mentioned in the Table, for 50°.

These methods are the most expeditious, and attended with the least trouble; but as ice may be used with equal certainty, and with much less expence, I shall give a particular detail of an experiment made with the use of it, first mentioning a preparatory experiment, to which I was immediately led by the recollection that Sir CHARLES BLAGDEN, in his paper "on the "point of congelation," (Phil. Trans. Vol. LXXVIII.) had found that common sal ammoniac and common salt, mixed with snow, produced a cold of — 12°, whereas the latter used alone with snow produces only — 5°. I used a mixed powder of equal parts of common sal ammoniac and nitre with the common salt, by which the thermometer sunk to — 18°; and when I used nitrous ammoniac with common salt, to — 25°; this cold I could not increase by the addition of any other salts, nor could I equal it by any other combination of salts: those I tried were GLAUBER's salt, salt of tartar, soda, and sal catharticus amarus; by several trials, I found the best proportions

to be, snow or pounded ice twelve parts, common salt five parts, and of nitrous ammoniac, or a powder of equal parts sal ammoniac and nitre mixed, five parts; or *one-third* of common salt, when I used that *alone*, with snow or pounded ice.

My apparatus then (Dec. 28th last) consisted of two vessels (fig. 3. and 4.); an instrument, (fig. 6.) to grind or rather scrape the ice to powder; a kind of spatula (I use a marrow-spoon) to stir the powder occasionally; a thermometer (fig. 8.); and a small thermometer glass with the bulb three-fourths full of quicksilver (fig. 7.). I filled the vessel, fig. 3, holding when inverted two pints, *stratum super stratum*, with pounded ice, common salt, and a powder consisting of equal parts sal ammoniac and nitre mixed together; by first putting in six ounces of pounded ice, then two ounces and a half of common salt, and, after stirring these well together, two ounces and a half of the mixed salts, mixing the whole well together; this was repeated in the same manner until the vessel was quite full; it was then tied over securely with a wet bladder, turned upright, and one ounce and a half of rain water poured into the tube through a funnel, the tube covered with a cork, and the vessel left undisturbed till the water was frozen perfectly solid. The instrument for grinding it was then put in to acquire cold, whilst the vessel, fig. 4, holding a pint, was filled in the same manner, with the same proportions of materials, a bladder tied over it, set upright, and one ounce of fuming nitrous acid poured in to be cooled. The ice was then ground to powder, and when finished, the nitrous acid being found to have acquired a sufficient degree of cold, *viz.* — 13°, the frigorific mixture of ice and salts was let out of the vessel which contained the nitrous acid; and the powdered ice (still sur-

rounded by its frigorific mixture) added to the acid as quick as possible; when the thermometer sunk to near — 50°, and the mixture soon froze the quicksilver in the glass bulb. In this experiment, 18 minutes were required to freeze the water perfectly solid; and 15 to reduce the ice, by moderate labour, to very fine powder. The experiment was over in 55 minutes; and the temperature of the preparatory cooling mixture then found to be — 10°.

I had a spirit thermometer by me, but a mercurial thermometer being much more sensible, and consequently descending much quicker, I prefer it in experiments made merely to freeze quicksilver; knowing from experience how the congelation is going on, from the irregular descent of the mercury when a few degrees below its freezing point; and from having usually found that the quicksilver in the thermometer glass begins to freeze, as soon as the mercurial thermometer reaches — 40°.

Whenever I have occasion to use ice in summer for this purpose, I usually pound together first some ice and salt in a stone mortar, about two parts of the former to one of the latter; throw this away, and wipe the pestle and mortar perfectly dry; the mortar being thus cooled, the ice may afterwards be pounded small without melting.

And as a mixture made of snow, or ice in powder, and salts, does not give out its greatest cold till it is become partially liquid, by the action of the ice and salts on each other; it is necessary that the whole be stirred well together, till it is become of an uniformly moist *pulpy* consistence, especially since in becoming liquid the mixture shrinks so much, that if this be not attended to the vessel will not be near full, and conse-

quently the upper part of the tube not surrounded, as it ought to be, by the frigorific mixture. The dissolution of the ice and salts may, if required, be hastened by adding occasionally a little water; but then the cold produced will be less intense, and not so durable.

That particular form of the vessel, in which the ice is made and reduced to powder, is chosen, because it subjects the powdered ice in the tube to the constant action of the freezing mixture, without which it would be less fit, particularly in warm weather, for the intended use, and because in it the ice is not liable to be impregnated with the salts of the mixture, by which it would be utterly spoiled: and *that* for cooling the nitrous acid, and making the second mixture in, because it is steady, and is besides insulated as it were from the external warm air, and surrounded in its stead by an atmosphere much colder.

It is scarcely necessary to add, that when snow which has never thawed can be procured, it may be cooled in this apparatus by a mixture of snow (instead of the pounded ice), and the salts, and the trouble of reducing the ice into powder saved.

I prefer the red fuming nitrous acid, because, as I have observed in a former paper, it requires no dilution. Being under the necessity at one time of using the pale nitrous acid, I found it required to be diluted with one-fifth its weight of water. The best and only way of trying or reducing any acid to the proper strength, is by adding snow, as Mr. CAVENDISH directs, or the powdered ice to it, until the thermometer cease to rise; then cool the acid to the same temperature of the snow again, add more snow, which will make the thermometer rise again, though less; cool it again, and

repeat this, until the addition of snow or powdered ice will not make the thermometer rise: to be very accurate, it should be reduced in this manner to the proper strength, at the temperature, whatever it be, at which the nitrous acid and snow, or powdered ice, are to be mixed together when cooled.

In the course of my experiments I have endeavoured to ascertain the comparative powers of ice to produce cold with nitrous acid, in the different forms I have had occasion to use it. The result is, that fresh snow sunk a thermometer to — 32°; ground ice to — 34°; and the most rare frozen vapour to below — 35°; the vessel and materials each time being + 30°.

The vessels for these mixtures, particularly that in which the quicksilver is to be frozen, should be thin, and made of the best conductors of heat; first, because thin vessels rob the mixture of less cold at mixing, *i. e.* if two mixtures of the same kind are made, one in a thin, the other in a thick vessel, the former will be coldest; secondly, because the air is a sufficiently bad conductor; and thirdly, for the very obvious reason, that the cold is transmitted through them quicker.

For these reasons, and from the difficulty I have found in procuring vessels of glass, which are undoubtedly fittest for experiments of this kind, I have used tin; which is readily had in any form, and if coated with wax, is sufficiently secured against the action of the acids.

I give the inside such a coating, by pouring melted white wax into the vessel, previously clean and dry, and turning it about by hand, so as to leave no point of the metal uncovered for the acid to act on, pouring the surplus away.

In the experiment above described, I used a single vessel for cooling the nitrous acid; a cupping-glass (represented by

the dotted line at *b*, fig. 4.) being cemented into the tin, and thereby forming that part in which the nitrous acid was first cooled, and the mixture afterwards made in which the quicksilver was frozen: but from the trouble and impediments arising from letting out the mixture, and clearing the bottom from the lumps of ice, &c. adhering to it, I was led to the addition of the other part (fig. 5.) by which all these difficulties are got rid of, and it is besides a much more comfortable and neat way of conducting it; the upper part which contains the nitrous acid being lifted off and placed on the table, immediately before the powdered ice is added.

The whole of this apparatus may be of tin, that part *only* (when the *cooling* mixtures are made *without* using any corrosive acid) in which the acid mixture is to be made, being previously coated in the manner above mentioned; or a thin glass tumbler of a proper size may be cemented in.

I have occasionally used a thin glass tumbler for the mixture in which the quicksilver is to be frozen, immersing it with the acid in a frigorific mixture till the acid is sufficiently cooled, then adding the ground ice to it, previously removing the tumbler out of the frigorific mixture, as in the experiment above mentioned; this simplifies the apparatus, but is less convenient on many accounts.

The scale of this apparatus may be diminished or increased at the will of the operator; for there is no doubt that a small quantity of quicksilver may be frozen at any time with one-fourth of this quantity, with an apparatus of this kind, by any one conversant in such experiments.

I have frequently frozen quicksilver, by mixing together, at 0°, three drams of ground ice with two drams of nitrous acid.

Whenever the intention is, as in these experiments, to cool the materials to *nearly* the same temperature with the frigorific mixture in which they are immersed, the proportion of the frigorific mixture to the intended mixture (or materials to be cooled) should not be *less* than twelve to one; a greater disproportion is still better.

By attending to the directions *particularly* mentioned in the experiment made on Dec. 28th, a thermometer may be *always* dispensed with; the proportions of the materials to be cooled being exactly adjusted; and *when* they are to be mixed precisely determined, by the time employed in grinding the ice to powder. The proportions of snow, or pounded ice, and salt, or salts, may be guessed sufficiently near without weighing, unless in very *nice* experiments.

Imagining that a recapitulation of the different mixtures, described in my former paper, for producing artificial cold, brought into one view might not be unuseful, I have subjoined a Table of the salts, their powers of producing cold with the different liquids, and the proportions of each, according to a careful repetition of each; the temperature being 50°.

Salts.	Liquor.		Temperature, or cold produced.
* Sal ammoniac 5, nitre 5	water	16	+10°
Sal ammoniac 5, nitre 5, GLAUBER's salt 8 - -	——	16	+4°
* Nitrous ammoniac 1 -	——	1	+4°
Nitrous ammoniac 1, sal soda 1	——	1	−7°
GLAUBER's salt 3 -	d. nitr. acid	2	−3°
GLAUBER's salt 6, sal ammoniac 4, nitre 2 - -	——	4	−10°
GLAUBER's salt 6, nitrous ammoniac 5 - -	——	4	−14°
Phosphorated soda 9 -	——	4	−12°
Phosphorated soda 9, nitrous ammoniac 6 - -	——	4	−21°
GLAUBER's salt 8 -	marine acid	5	−0°
GLAUBER's salt 5 -	d. vitr. acid	4	+3°

N. B. I have chosen the temperature of 50°, because the materials may at any time, by immersion in water drawn from a spring, be cooled nearly to that temperature, and the experiment for freezing with any of these mixtures commence there.

* The salts from each of these may be recovered by evaporating the mixture to dryness, and used again repeatedly.

N. B. The figures after each salt, and after the liquor, signify the proportion of parts, by Troy weight, to be used; the trouble of weighing the *water* may be saved by observing, that a full ounce of it by wine measure corresponds exactly with one ounce of it by Troy weight; likewise it must be noticed, when more kinds of salt than one are used, to add them to the liquor one after the other, in the order they stand in the Table: beginning on the left hand, and stirring the mixture well between each addition: d. nitr. acid, is red fuming nitrous acid two parts, and rain, or distilled water one part,

At a higher temperature than 50°, the quantity of the salts must be increased, and the effect will be proportionably greater; at a lower temperature diminished, when the effect will be proportionably less.

It must be observed, that to produce the greatest effect by any frigorific mixture, the salts should be fresh crystallized,* not damp, and newly reduced to very fine power; the vessel in which they are made very thin, and just large enough to contain the mixture; and the materials mixed intimately together, as quickly as possible, the proper proportions at any temperature (those in the Table being adjusted for the temperature of 50° *only)* having been *previously* tried, by adding the powdered salts gradually to the liquid, till the thermometer ceased to sink; observing to produce the full effect of one salt before a second is added, and likewise of the second before a third is added. Neither soda, phosphorated soda, nor Glauber's salt should be mixed with nitrous ammoniac, or the powder composed of sal ammoniac and nitre, unless at a low temperature, *i. e.* below 0°, but pounded and kept apart.

In the experiments alluded to in the Table, the precaution of fresh crystallizing the salts was not observed, because I chose to give the ordinary effects only; I therefore then used salts in their common state, taking care, however, to choose such as had not in the least effloresced.

Since it is always useful, and generally absolutely necessary,

by weight, well agitated together, and become cool: d. vitr. acid, is strong vitriolic acid, and rain, or distilled water, equal parts, by weight, thoroughly mixed (very cautiously) and cooled.

* Soda, phosphorated soda, and Glauber's salt, are best crystallized afresh, because their effect, especially the two last in the acids, depends upon the quantity of water they contain in a solid state.

to know how much room in a vessel the several materials take up separately, and when mixed, it will be right to observe, that snow, or ice in powder, at near 0°, occupy in measure nearly two-thirds more than their weight; that is, one ounce weight of water will, when in the form of snow, or ice ground to powder, nearly fill a vessel which holds three ounces wine measure; powdered salts nearly double their weight; strong nitrous acid about three-fourths its weight; and a mixture made of salts and diluted nitrous acid, measures rather less than two-thirds of the weight of the ingredients. Without a previous knowledge of this, it is impossible to adjust the size of the vessels to the mixtures which are to be made; because, in most nice experiments of this kind, the height to which a vessel will be filled is indispensably necessary to be known beforehand.

The long continuance of the late frost having afforded me opportunities of repeating these experiments in various ways, I shall mention briefly the result of such as appear to me to be material.

I have found, that ice may be ground so fine as to be equal to frozen vapour, and the harder it is frozen the finer it is ground, but with more labour:

That quicksilver may be frozen by cooling the nitrous acid only, saving the trouble and inconvenience of cooling the snow likewise; either by adding snow at + 32°, to nitrous acid at — 29°; or snow at + 25°, to nitrous acid at — 20°; or snow at + 20° to nitrous acid at — 12°; most winters offer an opportunity of doing it in this way; the nitrous acid may be cooled in a mixture of snow and nitrous acid:

That it may likewise be frozen, by mixing expeditiously together snow and nitrous acid, when the temperature of each is $+ 7°$:

Or by mixing ground ice and nitrous acid at $+ 10°$.

Hence it follows, that the cold of this climate offers occasionally opportunities of freezing quicksilver, without previously cooling by art the materials to be mixed; for I have once seen the thermometer at $+ 6°$, and others, I believe, have seen it lower.

I expected an opportunity would have offered this winter, but the lowest point I saw my thermometer at, this season, was only $+ 10°$; at this temperature, I mixed nitrous acid (cooled out of doors to the temperature of the air) and snow, on January 23d last; but the cold produced was not quite sufficient to freeze the quicksilver, although very near it, as indicated by a thermometer. From what I have observed *since* these latter experiments were made, I think it may be reasonably expected, that powdered ice and nitrous acid at $+ 14°$, or snow at $+ 10°$, will succeed, if mixed expeditiously.

Strong spirit of vitriol, whose specific gravity is 1,848, required to be diluted with half its weight of water, and produced with snow at the temperature of $+ 30°$, about eight degrees less than with nitrous acid, sinking the thermometer to $- 24°$; four parts of the diluted vitriolic acid required, at that temperature, six parts of snow.

It perhaps will be remarked, that I have taken no notice before of the vitriolic acid. The reason is, because the freezing point of quicksilver being 39°, it may be frozen tolerably hard by a mixture of nitrous acid with snow, or ground ice, though

the utmost degree of cold this acid can produce with snow is — 46°; which degree of cold may be produced by mixing the snow or ground ice and nitrous acid at 0°.

If it be required to make it perfectly solid and hard, a mixture of equal parts of the diluted vitriolic acid and nitrous acid should be used with the powdered ice, but then the materials should not be less than — 10° before mixing.

If a still greater could be required than a mixture of this kind can give, which is about — 56°, the diluted vitriolic acid *alone* should be used with snow or powdered ice, and the temperature at which the materials are to be mixed not less than — 20°.

Select, according to the intention, either of the three following mixtures:

First, snow or pounded ice two parts, and common salt one part, which produces a cold of — 5°:

Second, snow or pounded ice twelve parts, common salt five parts, and a powder, consisting of equal parts of common sal ammoniac and nitre mixed, five parts, which produces a cold of — 18°:

Third, snow or pounded ice twelve parts, common salt five parts, and nitrous ammoniac in powder five parts, which produces a cold of — 25°.

The proportions which I have found to be the best for mixing the snow or powdered ice with the different acids, at different temperatures, are these; *viz.* at + 30°, seven of the former to four of the nitrous acid; at + 5° (with a trifling allowance, if any, for a few degrees above or below), three to two; at — 12°, four to three, with the mixed acids; and at — 20°, with the diluted vitriolic acid, equal parts.

If it be required to prepare the materials in a frigorific mixture, without the use of *ice*, a mixture of the proper strength may be chosen from the Table.

It is immaterial, when the exact proportions of each are known, whether the powdered ice be added to the acid, or the acid poured upon that, provided the powdered ice be kept stirred to prevent lumps forming, and the materials be mixed as quick as possible. But when the proportion is not known, it is better to be provided with more powdered ice than is expected to be wanted; and add it to the acid by degrees, until the greatest effect is produced, as shewn by a thermometer.

The consistence is a pretty sure guide to those accustomed to mixtures of this kind; *viz.* when fresh additions of snow or ice do not readily dissolve in the acid, though well stirred, and the mixture acquires a thickish flocculent appearance.

Snow, or powdered ice, that have ever been subjected to a cold less than freezing are spoiled, or rendered much less fit for experiments of this kind.

I prefer the method of adding the powdered ice or snow to the acid in a *separate* vessel, principally because the size of that vessel may be *exactly* adjusted to the quantity of mixture it is to contain.

A mixture made of diluted nitrous acid, phosphorated soda, and nitrous ammoniac (by much the most powerful of any compounded of *salts* with acids), prepared with the greatest accuracy, is not quite equal to a mixture of snow and nitrous acid, each mixed at $+ 30°$, although very nearly so.

Though quicksilver may be frozen by salts dissolved in acids, it is necessary that the materials be cooled, previously to mixing, much lower than when snow or ground ice are used.

If it be required to mix the powdered salts and acids at a low temperature, the best method is this: put first the nitrous ammoniac into the tube of such an apparatus as fig. 1. shaking it down level, gently pressing the upper surface smooth; then the phosphorated soda or GLAUBER's salt; cover this with a circular piece of writing paper, and pour a little melted white wax upon it, and when cold, pour upon this the diluted nitrous acid; immerse this in a frigorific mixture till it is sufficiently cold, as found by dipping the thermometer into the liquor occasionally; force a communication through, and stir the whole thoroughly together, contriving that the upper stratum of salt, that is, the phosphorated soda or GLAUBER's salt, be mixed with the liquor first, and then the nitrous ammoniac; the powdered salts do not require stirring whilst cooling, like snow, for however hard they are frozen, they will readily dissolve in the acid; care must be taken that the partition be perfect between the salts and the liquor; and that in this, and every instance where the materials are to be cooled, they be immersed *below* the surface of the frigorific mixture. The strength of the red fuming nitrous acid used in these experiments, I found to be 1,510, and that of the vitriolic acid 1,848.

I have thought it better, for the sake of brevity, *not* to use in this, as in my former papers, the new chemical names, especially as the old ones are more generally known.

These experiments were chiefly made in a warm room, not far from the fire side.

I have now finished my proposed plan respecting the best modes of conducting experiments on cold, in which it will appear, that I have reduced the congelation of quicksilver, in

any climate at any season, to as certain, and almost as easy a process, as that I originally set out with, for the freezing of water (Phil. Trans. Vol. LXXVII.); *viz.* by previously cooling the materials in one mixture, to produce the effect in a second. It may very likely appear to some, that I have been too minute in a few particulars; yet as perhaps experiments of this kind, all circumstances considered, are inferior to few in the delicacy required to make them succeed completely, I trust I shall be excused by those who choose to repeat them, particularly such as are not in the habit of making experiments of this kind; especially if it secure them from an unsuccessful attempt, and that, perhaps, without being able to account for it.

Oxford,
March 1st, 1795.

It is very well known, that vitriolic ether will produce sufficient cold by evaporation to freeze water; this circumstance is noticed by many, and several different methods have been proposed, particularly one by Mr. CAVALLO, with a very ingenious apparatus for the purpose (Phil. Trans. Vol. LXXI.); nevertheless, as I am upon the same subject, and the following experiments differ, as well in the effect produced as in the particular mode of conducting them, from any I have met with, I have ventured to mention them.

June 29th, 1792, temperature of the air 71°, I sunk a thermometer (the bulb being covered with fine lint tied over it, and clipped close round), by dipping it in ether, and fanning it, to 26°; then, by exposing the thermometer to the brisk thorough air of an open window, to 20°; and again, by using

some of the same ether, but which had been purified by agitating it with eight times its weight of water, applied exactly as in the last experiment, the thermometer sunk to 12°. Water tried in the same manner, at the same temperature, sunk the thermometer to 56°.

A whirling motion was given the thermometer during each experiment.

The lint was renewed for each experiment, and the bulb required to be dipped into the ether thrice; the first time sufficiently to soak it, after which the thermometer was held at the window till it ceased to sink; then a second *quick* immersion, and likewise a third, exposing the thermometer in like manner after *each* immersion.

In this manner a little water in a small tube may be frozen presently, by good ether *not purified*, at any time, especially if a small wire be used to scratch or scrape the sides of the tube, below the surface of the water.

During the warmest weather of last summer I frequently froze water in this way.

EXPLANATION OF THE PLATE. (Tab. XXIII.)

Fig. 1. is a vessel in one piece, open at the bottom; *a, a,* the body, holding inverted two pints; *b,* the tube, holding five ounces; the lower or smaller part (formed by a contraction, or lessening of the tube in diameter, merely for the purpose of leaving a small shoulder for a temporary partition), holding rather *less* than one-fifth of the whole.

Fig. 2. is a vessel consisting of two parts; *a, a,* the body,

holding two pints; *b*, the tube, holding five ounces, which, together with the lid *c*, forms a cover to take off and on the vessel.

N. B. This vessel may, if preferred, be used instead of fig. 1. the parts corresponding with it, except in not being open at bottom, and the continuation of the tube upwards *just sufficient* to serve for a handle.

Fig. 3. is a vessel in one piece, open at the bottom, holding when inverted two pints; *b*, the tube, holding four ounces and a half.

Fig. 4. a vessel open at bottom, holding inverted one pint.

Fig. 5. a cover to fig. 4. *a, a,* the body, fitting exactly over, and *b* the cup-part (holding three ounces), fitting exactly within, the corresponding parts of fig. 4.

Fig. 6. the instrument for grinding the ice into powder; it works upon a short centre point, and has the edge bevilled contrary ways on each side the point, so as to follow. The fineness of the powder is regulated by the degree of pressure used. The handle is wood, the rest metal: *a*, is a sliding cover, fitting on the tube in which the ice is ground, to exclude the external air, and to keep the instrument steady; *b*, is the shoulder or guard, to prevent the point of the instrument from touching, so as to endanger injuring the bottom of the tube. It should be made so as to fit, without grating the inside of the tube in using.

The tubes of each of the vessels should be somewhat shorter than the vessel, so as not quite to reach the bottom of it.

Fig. 7. a thermometer glass, with the bulb three-fourths full of quicksilver.

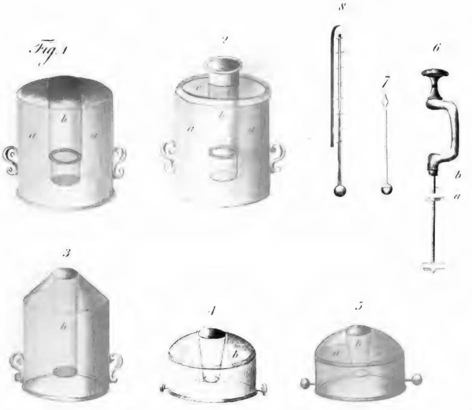


Fig. 1
a
b
a
2
c
a
b
a
8
7
6
b
a
3
b
4
b
5
a
b

Fig. 8. a thermometer, with the lower part of the scale-board turned up with a hinge, for the convenience of taking the temperature of small quantities, or of mixtures in which mineral acids form a part.

N. B. These vessels are represented as in glass, that being undoubtedly fittest for purposes in which corrosive acids are to be used.

XIV. *Observations on the Grafting of Trees. In a Letter from* Thomas Andrew Knight, *Esq. to Sir* Joseph Banks, *Bart. P. R. S.*

Read April 30, 1795.

SIR,

I AM encouraged to address the following letter to you, by the opinion you were last year pleased to express of part of my experiments and observations, on the diseases and decay of those varieties of the apple and pear which have been long in cultivation. The disease from whose ravages they suffer most is the canker, the effects of which are generally first seen in the winter, or when the sap is first rising in the spring. The bark becomes discoloured in spots, under which the wood, in the annual shoots, is dead to the centre, and in the older branches, to the depth of the last summer's growth. Previous to making any experiments, I had conversed with several planters, who entertained an opinion, that it was impossible to obtain healthy trees of those varieties which flourished in the beginning and middle of the present century, and which now form the largest orchards in this country. The appearance of the young trees, which I had seen, justified the conclusion they had drawn; but the silence of every writer on the subject of planting, which had come in my way, convinced me that it was a vulgar error, and the following experiments were undertaken to prove it so.

I suspected that the appearance of decay in the trees I had seen lately grafted, arose from the diseased state of the grafts, and concluded, that if I took scions or buds from trees grafted in the year preceding, I should succeed in propagating any kind I chose. With this view I inserted some cuttings of the best wood I could find in the old trees, on young stocks raised from seed. I again inserted grafts and buds taken from these on other young stocks, and wishing to get rid of all connection with the old trees, I repeated this six years; each year taking the young shoots from the trees last grafted. Stocks of different kinds were tried, some were double grafted, others obtained from apple-trees which grew from cuttings, and others from the seed of each kind of fruit afterwards inserted on them; I was surprised to find that many of these stocks inherited all the diseases of the parent trees.

The wood appearing perfect and healthy in many of my last grafted trees, I flattered myself that I had succeeded; but my old enemies, the moss and canker, in three years convinced me of my mistake. Some of them, however, trained to a south wall, escaped all their diseases, and seemed (like invalids) to enjoy the benefit of a better climate. I had before frequently observed, that all the old fruits suffered least in warm situations, where the soil was not unfavourable. I tried the effects of laying one kind, but the canker destroyed it at the ground. Indeed I had no hopes of success from this method, as I had observed that several sorts which had always been propagated from cuttings, were as much diseased as any others. The wood of all the old fruits has long appeared to me to possess less elasticity and hardness, and to feel more soft and spongy under the knife, than that of the new varieties

which I have obtained from seed. This defect may, I think, be the immediate cause of the canker and moss, though it is probably itself the effect of old age, and therefore incurable.

Being at length convinced that all efforts, to make grafts from old and worn out trees grow, were ineffectual, I thought it probable that those taken from very young trees, raised from seed, could not be made to bear fruit. The event here answered my expectation. Cuttings from seedling apple-trees of two years old were inserted on stocks of twenty, and in a bearing state. These have now been grafted nine years, and though they have been frequently transplanted to check their growth, they have not yet produced a single blossom. I have since grafted some very old trees with cuttings from seedling apple-trees of five years old: their growth has been extremely rapid, and there appears no probability that their time of producing fruit will be accelerated, or that their health will be injured, by the great age of the stocks. A seedling apple-tree usually bears fruit in thirteen or fourteen years; and I therefore conclude, that I have to wait for a blossom till the trees from which the grafts were taken attain that age, though I have reason to believe, from the form of their buds, that they will be extremely prolific. Every cutting, therefore, taken from the apple (and probably from every other) tree, will be affected by the state of the parent stock. If that be too young to produce fruit, it will grow with vigour, but will not blossom; and if it be too old, it will immediately produce fruit, but will never make a healthy tree, and consequently never answer the intention of the planter. The root, however, and the part of the stock adjoining it, are greatly more durable than the bearing branches; and I have no doubt but that scions obtained from

either would grow with vigour, when those taken from the bearing branches would not. The following experiment will at least evince the probability of this in the pear-tree. I took cuttings from the extremities of the bearing branches of some old ungrafted pear-trees, and others from scions which sprang out of the trunks near the ground, and inserted some of each on the same stocks. The former grew without thorns, as in the cultivated varieties, and produced blossoms the second year; whilst the latter assumed the appearance of stocks just raised from seeds, were covered with thorns, and have not yet produced any blossoms.

The extremities of those branches, which produce seeds in every tree, probably shew the first indication of decay; and we frequently see (particularly in the oak) young branches produced from the trunk, when the ends of the old ones have long been dead. The same tree when cropped will produce an almost eternal succession of branches. The durability of the apple and pear, I have long suspected to be different in different varieties, but that none of either would vegetate with vigour much, if at all, beyond the life of the parent stock, provided that died from mere old age. I am confirmed in this opinion by the books you did me the honour to send me: of the apples mentioned and described by PARKINSON, the names only remain, and those since applied to other kinds now also worn out; but many of EVELYN's are still well known, particularly the red-streak. This apple, he informs us, was raised from seed by Lord SCUDAMORE in the beginning of the last century.* We have many trees of it, but they appear to have been in a state of decay during the last forty

* Probably about the year 1634.

years. Some others mentioned by him are in a much better state of vegetation; but they have all ceased to deserve the attention of the planter. The durability of the pear is probably something more than double that of the apple.

It has been remarked by EVELYN, and by almost every writer since, on the subject of planting, that the growth of plants raised from seeds was more rapid, and that they produced better trees than those obtained from layers or cuttings. This seems to point out some kind of decay attending the latter modes of propagation, though the custom in the public nurseries of taking layers from stools (trees cropped annually close to the ground) probably retards its effects, as each plant rises immediately from the root of the parent stock.

Were a tree capable of affording an eternal succession of healthy plants from its roots, I think our woods must have been wholly over-run with those species of trees which propagate in this manner, as those scions from the roots always grow in the first three or four years with much greater rapidity than seedling plants. An aspin is seldom seen without a thousand suckers rising from its roots; yet this tree is thinly, though universally, scattered over the woodlands of this country. I can speak from experience, that the luxuriance and excessive disposition to extend itself in another plant, which propagates itself from the root (the raspberry), decline in twenty years from the seed. The common elm being always propagated from scions or layers, and growing with luxuriance, seems to form an exception; but as some varieties grow much better than others, it appears not improbable that the most healthy are those which have last been obtained from seed. The different degrees of health in our peach and nec-

tarine trees may, I think, arise from the same source. The oak is much more long-lived in the north of Europe than here; though its timber is less durable, from the numerous pores attending its slow growth. The climate of this country being colder than its native, may in the same way add to the durability of the elm; which may possibly be further increased by its not producing seeds in this climate, as the life of many annuals may be increased to twice its natural period, if not more, by preventing their seeding.

I have been induced to say a great deal more on this subject than, I fear, you will think it deserves, from a conviction that immense advantages would arise from the cultivation of the pear and apple in other counties, and that the ill success which has attended any efforts to propagate them, has arisen from the use of worn out and diseased kinds. Their cultivation is ill understood in this country, and worse practised; yet an acre of ground, fully planted, frequently affords an average produce of more than five hundred gallons of liquor, with a tolerably good crop of grass; and I have not the least doubt but that there are large quantities of ground in almost every county in England capable of affording an equal produce.

I have only to add an assurance, that the results of the foregoing experiments are correctly stated; and that

I am, Sir, &c.

Elton, Herefordshire,
April 13, 1795.

THO. AND. KNIGHT.

XV. *On Welding Cast Steel. By Sir* Thomas Frankland, *Bart. F. R. S.*

Read May 21, 1795.

THE uniting of steel to iron by *welding* is a well known practice; in some cases for the purpose of saving steel, in others to render work less liable to break, by giving the steel a back, or support, of a tougher material.

Ever since the invention of *cast steel* (or bar steel refined by fusion), it has generally been supposed impossible to weld it either to common steel, or iron; and naturally, for the description in WATSON's Chemical Essays (Vol. IV. page 148) is just, that in a welding heat it "runs away under the hammer like sand." How far the Sheffield artists, who stamp much low-priced work with the title of cast steel, practise the welding it, I am ignorant; but though I have inquired of many smiths and cutlers in different parts of the kingdom, I have not yet found the workman who professed himself able to accomplish it. If, therefore, I should describe a simple process for the purpose, I may be of use to the very many who are incredulous on the subject.

If any one has made the discovery on principle, he has reasoned thus: cast steel in a welding heat is too soft to bear being hammered; but is there no lower degree of heat in which it may be soft enough to unite with iron, yet without

hazard of running under the hammer? A few experiments decided the question; for the fact is, that cast steel in a *white heat*, and iron in a *welding heat*, unite completely.

It must not be denied that considerable nicety is required in giving a proper heat to the steel; for on applying it to the iron it receives an increase of heat, and will sometimes run on that increase, though it would have borne the hammer in that state in which it was taken from the fire.

I need scarcely observe, that when this process is intended, the steel and iron must be heated separately, and the union of the parts proposed to be joined effected at a single heat. In case of a considerable length of work being required, a suitable thickness must be united, and afterwards drawn out, as is practised in forging reap-hooks, &c.

The steel on which my experiments have been made are WALKER's of Rotherham, and HUNTSMAN's, between which I discover no difference; and though there may be some trifling variation in the flux used for melting, they are probably the same in essentials.

December, 1794.

XVI. *The Binomial Theorem demonstrated by the Principles of Multiplication.* *By* Abram Robertson, *A. M. of* Christ Church, Oxford, *F. R. S.* *In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. and Astronomer Royal.*

Read May 21, 1795.

REV. SIR, Christ Church, Oxford, Oct. 27th, 1794.

A CONSIDERATION of the very high importance and extensive utility of the binomial theorem, having induced me to enter upon an examination of the methods in which, at different times, it has been demonstrated; and having frequently reviewed them, and deliberated with myself upon the subject, I was convinced that a demonstration begun and conducted upon the obvious principles of multiplication was still wanted, much to be desired, and also attainable. For to these principles involution must be ultimately referred, in whatever form it may be presented; and it therefore appeared, that an investigation of the theorem effected by them only, was likely to be as simple and perspicuous as the subject will permit.

I think it needless to enter into a minute account of the demonstrations heretofore published, or to enumerate the objections which have been or may be made to them. It is well known to mathematicians that they are effected either by induction, by the summation of figurate numbers, by the

doctrine of combinations, by assumed series, or by fluxions: but that multiplication is a more direct way to the establishment of the theorem than any of these, cannot, I suppose, be doubted. Proceeding by it, we have always an evident first principle in view, to which, without the aid of any doctrine foreign to the subject, we can appeal for the truth of our assertions, and the certainty and extent of our conclusions.

The following demonstration, which owes its origin to the abovementioned train of thinking, might be divided into two parts; but I thought it more advisable to divide it into articles, and number them for the sake of references. That which might be called the first part, extends from the first to the end of the twelfth article, and contains the investigation of the theorem, as far as it relates to the raising of integral powers. The remaining articles constitute the second part, which contains the demonstration of the theorem as applicable to the extraction of roots, or the raising of powers, when the exponents are vulgar fractions. If the assumption of the series, in which the theorem is usually expressed, be allowed, the first part might be inferred as a corollary from the demonstration of the second. For having proved that $\overline{x+z}^{\frac{n}{r}} = x^{\frac{n}{r}} + \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} +$, &c. it follows, that when r is equal to 1, then $\overline{x+z}^n = x^n + nzx^{n-1} + n \times \frac{n-1}{2} \times z^2 x^{n-2} +$, &c. I could not, however, think of suppressing the first part, as the binomial series is so easily investigated in it from first principles.

Upon examining the Philosophical Transactions, I found a demonstration of this important theorem by CASTILLIONEUS,

in the XLIId Volume. In effecting it he had recourse to the doctrine of combinations of quantities, in that part of his investigation which relates to the raising of integral powers; and by extending this to the involution of a multinomial, and employing an assumed series, he made out the most general case, or that in which the exponent is a fraction. In neither of the cases, however, in my opinion, is the law of continuation proved with sufficient perspicuity. In the XLVIIth Vol. of the Transactions there is a paper, not expressly on the binomial theorem, by the celebrated Mr. THOMAS SIMPSON, in which the case for raising integral powers is demonstrated by fluxions.

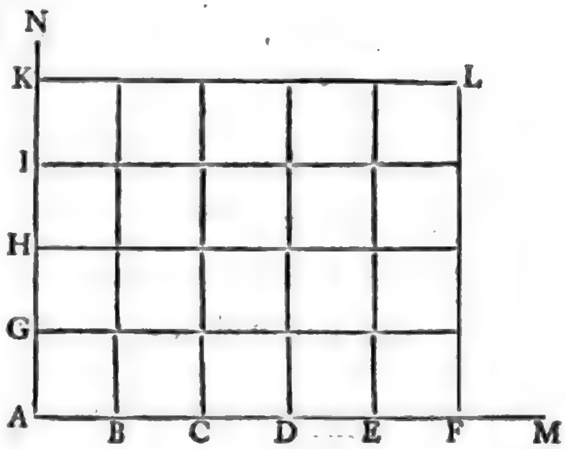
With respect to the following demonstration, I submit it to your inspection, with the most perfect confidence in your judgment and candour; and if it appears to you not unworthy of the attention of the Royal Society, by presenting it to that learned body you will add to the favours which you have already conferred upon me.

I am, &c.

A. ROBERTSON.

1. The product arising from the multiplication of any number of quantities * into one another, continues the same in value, in every variation which may be made in the arrangement of the quantities which compose it. Thus $p \times q \times r \times s = pqrs = spqr = psqr = pqsr =$ any other arrangement of the same quantities.

* When I speak of the multiplication of quantities into one another, I mean the multiplication of the numbers into one another which measure those quantities.



For let AM, AN be two indefinite straight lines at right angles to one another, and in AM set off AB, BC, CD, DE, EF, &c. equal to one another, and in number equal to the number of units in the quantity p*; and in AN set off AG, GH, HI, IK, &c. each equal to AB, and let the number of these parts be equal to the number of units in the quantity q. Complete the rectangle KF, and draw straight lines parallel to AK, through the points B, C, D, E, and let them meet the opposite side KL of the parallelogram. Through the points G, H, I, draw straight lines parallel to AF, and let them meet FL, the opposite side of the parallelogram. Then will the whole rectangle KF be divided into squares, each equal to GB. Now when p is multiplied into q, the number of units in the product is equal to the number of units in p repeated as often as there are units in q. But the number of squares in the rectangle KF is equal to the number of parts in AF repeated as often as there are parts in AK; and therefore, by the above construction, the number of squares in the rectangle KF is equal to the number of units in p repeated as often as there are units in q. Hence the number of squares in the rectangle KF is equal to the number of units in $p \times q$. In the same manner it may be proved that the number

* When I speak of the number of units in a quantity, I mean the number of units in the number measuring that quantity.

of squares in the rectangle K F is equal to the number of units $q \times p$; and consequently $pq = qp$.

Hence it follows that $pqrs = spqr$; for by the above, $pqr \times s = s \times pqr$. Also $spqr$ is equal to $psqr$; for $spqr = sp \times q \times r =$ (by the above) $ps \times q \times r$. Again, $psqr = pqsr$; for $psqr = p \times sqr = p \times qsr = pqsr$, by the above. And if $x + a = p$, $x + b = q$, $x + c = r$, $x + d = s$, $x + e = t$, &c. then $\overline{x+a} \times \overline{x+b} \times \overline{x+c} \times \overline{x+d} \times \overline{x+e} = pqrst = \overline{x+a} \times \overline{x+b} \times \overline{x+c} \times \overline{x+e} \times \overline{x+d} = pqrts =$ any other arrangement which can take place in the quantities.

2. It is evident that each of the quantities a, b, c, &c. will be found the same number of times in the compound product arising from $\overline{x+a} \times \overline{x+b} \times \overline{x+c} \times \overline{x+d} \times \overline{x+e}$, &c. For this product is equal to $pqrst = pqrs \times \overline{x+e} = pqrt \times \overline{x+d} = pqst \times \overline{x+c} = prst \times \overline{x+b} = qrst \times \overline{x+a}$, by substituting for the compound quantities, $x + a$, $x + b$, &c. their equals p, q, &c. Wherefore, in the compound product, each of the quantities a, b, c, &c. will be found multiplied into the products of all the others.

3. These things being premised, we may proceed to the multiplication of the compound quantities $x + a$, $x + b$, $x + c$, &c. into one another; and in order to be as clear as possible in what follows, let us consider the sum of the quantities, $a, b, c,$ &c. or the sum of any number of them multiplied into one another, as coefficients to the several powers of x, which arise in the multiplication. By considering products which contain the same number of the quantities a, b, c, &c. as homologous, the

multiplication will appear as follows, and equations of various dimensions will arise, according to the powers of x.

$$\begin{array}{l} x + a = p \\ x + b = q \\ \hline \end{array}$$

$$x^2 \left.\begin{array}{l} +a \\ +b \end{array}\right\} x + ab = pq;$$ a quadratic equation.

$$\begin{array}{l} x + c = r \\ \hline \end{array}$$

$$x^3 \left.\begin{array}{l} +a \\ +b \\ +c \end{array}\right\} x^2 \left.\begin{array}{l} +ab \\ +ac \\ +bc \end{array}\right\} x + abc = pqr;$$ a cubic.

$$\begin{array}{l} x + d = s \\ \hline \end{array}$$

$$x^4 \left.\begin{array}{l} +a \\ +b \\ +c \\ +d \end{array}\right\} x^3 \left.\begin{array}{l} +ab \\ +ac \\ +bc \\ +ad \\ +bd \\ +cd \end{array}\right\} x^2 \left.\begin{array}{l} +abc \\ +abd \\ +acd \\ +bcd \end{array}\right\} x + abcd = pqrs;$$ a biquadratic.

$$\begin{array}{l} x + e = t \\ \hline \end{array}$$

$$x^5 \left.\begin{array}{l} +a \\ +b \\ +c \\ +d \\ +e \end{array}\right\} x^4 \left.\begin{array}{l} +ab \\ +ac \\ +bc \\ +ad \\ +bd \\ +cd \\ +ae \\ +be \\ +ce \\ +de \end{array}\right\} x^3 \left.\begin{array}{l} +abc \\ +abd \\ +acd \\ +bcd \\ +abe \\ +ace \\ +bce \\ +ade \\ +bde \\ +cde \end{array}\right\} x^2 \left.\begin{array}{l} +abcd \\ +abce \\ +abde \\ +acde \\ +bcde \end{array}\right\} x + abcde = pqrst;$$ a sursolid.

&c.

4. From the above it appears, that the coefficient of the highest power of x in any equation is 1; but the coefficient of any other power of x in the same equation consists of a certain number of members, each of which contains one, two, three, &c.

of the quantities of a, b, c, &c. Thus the coefficient of the third term of any equation, is made up of members, each of which contains two of the quantities only, as, $ab + ac + bc$, the coefficient of the third term in the cubic equation. And indeed, not only from inspection, but also from considering the manner in which the equations are generated, it is evident, that each member of any coefficient has as many of the quantities in it, as there are terms in the equation preceding the term to which it belongs. Thus, $abc + abd + acd + bcd$ is the coefficient of the fourth term in the biquadratic, each of the members has three quantities in it, and three terms precede that to which they belong.

5. When any equation is multiplied in order to produce the equation next above it, it is evident that the multiplication by x produces a part in the equation to be obtained, which has the same coefficients as the equation multiplied. Thus, multiplying the cubic equation by x we obtain that part of the biquadratic which has the same coefficients as the cubic: the only effect of this multiplication being the increase of the exponents of x by 1.

6. But when the same equation is multiplied by the quantity adjoined to x by the sign $+$, each term of the product, in order to rank under the same power of x, must be drawn one term back. Thus when the first term of the cubic is multiplied by d, the product must be placed in the second term of the biquadratic. When the second term of the cubic is multiplied by d, the product must be placed in the third term of the biquadratic: and so of others.

7. As the equation last produced is the product of all the compound quantities $x + a$, $x + b$, $x + c$, &c. into one ano-

ther, and as it was proved in the second article that each of the quantities *a*, *b*, *c*, &c. must be found the same number of times in this product, if we can compute the number of times any one of those quantities enters into the coefficient of any term of the last equation, we shall then know how often each of the other enters into the same coefficient: and this may be done with ease, if of the quantities *a*, *b*, *c*, &c. we fix upon that used in the last multiplication. For the last equation, and indeed any other, may be considered as made up of two parts; the first part being the equation immediately before the last multiplied by x, according to the 5th article, and the other being the same equation multiplied by the quantity adjoined to x by the sign $+$, last used in the multiplication, according to the 6th article. This last used quantity, therefore, never enters into the members of the coefficient of the first of these two parts, but it enters into all the members of the coefficients of the last of them. But that part into which it does not enter has the same members as the coefficients of the equation immediately before the last, by the 5th article; and when the members of the first part are multiplied by the last used quantity, the product becomes the second part of the whole coefficient above mentioned.

Thus the first part of the cubic equation, by the 5th article is, $x^3 \begin{matrix} +a \\ +b \end{matrix}\Big\} x^2 + abx$, and as these coefficients are the same as the coefficients in the quadratic equation, being multiplied by c, and arranged according to the 6th article, we have the coefficients of the second part of the cubic, viz. $c \begin{matrix} +ac \\ +bc \end{matrix} + abc.$

Hence it is evident, that there are as many members in any

coefficient, which have the last used quantity in them, as there are members in the coefficient preceding, which have not the same quantity; and as it has been proved that each of the quantities a, b, c, &c. enters the same number of times into the coefficient of the same term, what has here been proved of the last used is applicable to each.

8. From the last article the number of members in the several coefficients of any equation may be determined. For if we put $s =$ the number of times each quantity is found in a coefficient, $n =$ the number of quantities a, b, c, &c. and $p =$ the number of quantities in each member; then as a is found s times in this coefficient, b is found s times in this coefficient, &c. the number of quantities in this coefficient, with their repetitions, will be $s \times n$, and as p expresses the number of quantities requisite for each member, the number of members in the coefficient will be $\frac{sn}{p}$.

9. Using the same notation, we can, by the last two articles, calculate the number of members in the next coefficient. For as $\frac{sn}{p}$ expresses the number of members in the abovementioned coefficient, and s the number of times each quantity is found in it, $\frac{sn}{p} - s =$ the number of times each is not found in it. By the 6th article, therefore, a will be found $\frac{sn}{p} - s$ times, b will be found $\frac{sn}{p} - s$ times, &c. in the next coefficient, and $\overline{\frac{sn}{p} - s} \times n$ $= \frac{sn^2 - psn}{p} =$ the number of quantities, with their repetitions, in it. But as the number of quantities in each member of a coefficient is 1 less than the number in each member of the coefficient next following, each member of the coefficient

whose number of members we are now calculating will have in it $p+1$ number of quantities. Consequently $\frac{sn^2-psn}{p\times p+1}=\frac{sn}{p}\times\frac{n-p}{p+1}=$ the number of members of the coefficient next after that whose number of members is $\frac{sn}{p}$, as in the last article.

The same conclusion may be obtained in the following manner. Let $m=$ the number of members in a coefficient, $p=$ the number of quantities in each member, and $n=$ the number of quantities $a, b, c,$ &c. Then will mp express the number of quantities with their repetitions in this coefficient, and $\frac{mp}{n}$ the number of times each quantity is found in it. Hence, as each quantity is only found once in the same member, $m-\frac{mp}{n}=$ the number of times each is not found in this coefficient, and is therefore equal to the number of times each is found in the next coefficient, according to the 6th article. The number of quantities, therefore, with their repetitions, in the next coefficient is expressed by $\overline{m-\frac{mp}{n}}\times n=mn-mp$; and as the number of quantities in each of its members is denoted by $p+1$, the number of its members is expressed by $\frac{mn-mp}{p+1}=m\times\frac{n-p}{p+1}$.

10. The binomial theorem, as far as it relates to the raising of integral powers, easily follows from the foregoing articles. For if all the quantities $a, b, c,$ &c. used in the multiplication in the 3d article, be equal to one another, and consequently each equal to a, each of the members in any coefficient will become a power of a; and each term in an equation will consist of a power of a multiplied into a power of x, having such a numeral coefficient prefixed as expresses the num-

ber of members in the coefficient, when exhibited in the manner of the 3d article. And as n expressed the number of quantities a, b, c, &c. used in the multiplication, when each of these quantities is equal to a, it will denote the power of the binomial $x + a$.

Hence, if m denote the numeral coefficient of any term of the nth power of $x + a$, and p the exponent of a in that term, the numeral coefficient of the next term will be expressed by $m \times \frac{n-p}{p+1}$, as is evident from the last article.

11. It is manifest from the 3d article that $x + a$ being raised to the nth power, the series, without the numeral coefficients, will be $x^n + a\,x^{n-1} + a^2\,x^{n-2} + a^3\,x^{n-3} +$, &c. and as the coefficient of the first term is 1, and of the second n, from the general expression in the last article $\overline{x+a}^n = x^n + n\,a\,x^{n-1} + n \times \frac{n-1}{2}\,a^2\,x^{n-2} + n \times \frac{n-1}{2} \times \frac{n-2}{3}\,a^3\,x^{n-3} +$, &c.

12. If equations be generated from $\overline{x-a} \times \overline{x-b} \times \overline{x-c} \times \overline{x-d}$, &c. the coefficients will be the same, excepting the signs, as those which result from $\overline{x+a} \times \overline{x+b} \times \overline{x+c} \times \overline{x+d}$, &c. in the 3d article; and as $- \times -$ gives $+$, but $- \times - \times -$ gives $-$, the coefficients, in equations generated from $\overline{x-a} \times \overline{x-b} \times \overline{x-c} \times \overline{x-d}$, &c. whose members have each an even number of quantities will have the sign $+$, but coefficients whose members have each an odd number of quantities will have the sign $-$. And hence it is evident that $\overline{x-a}^n = x^n - n\,a\,x^{n-1} + n \times \frac{n-1}{2}\,a^2\,x^{n-2} - n \times \frac{n-1}{2} \times \frac{n-2}{3}\,a^3\,x^{n-3} +$, &c.

13. Having thus investigated the binomial theorem, as far

as it relates to the raising of integral powers, I proceed to demonstrate, by the principles of multiplication, the most general case; *viz.* that $\overline{x+z}|^{\frac{n}{r}} = x^{\frac{n}{r}} + \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2}$ +, &c. This will clearly appear after it has been proved that if the series $x^{\frac{n}{r}} + \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} \times \frac{\frac{n}{r}-2}{3} z^3 x^{\frac{n}{r}-3}$ +, &c. be multiplied by the series $x^{\frac{1}{r}} + \frac{1}{r} z x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} z^2 x^{\frac{1}{r}-2} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} \times \frac{\frac{1}{r}-2}{3} z^3 x^{\frac{1}{r}-3}$ +, &c. the product will be $x^{\frac{n+1}{r}} + \frac{n+1}{r} z x^{\frac{n+1}{r}-1} + \frac{n+1}{r} \times \frac{\frac{n+1}{r}-1}{2} z^2 x^{\frac{n+1}{r}-2} + \frac{n+1}{r} \times \frac{\frac{n+1}{r}-1}{2} \times \frac{\frac{n+1}{r}-2}{3} z^3 x^{\frac{n+1}{r}-3}$ +, &c. Or, which is the same thing, after it has been proved that if the series $x^{\frac{n}{r}} + \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{n-r}{2r} z^2 x^{\frac{n-2r}{r}} + \frac{n}{r} \times \frac{n-r}{2r} \times \frac{n-2r}{3r} z^3 x^{\frac{n-3r}{r}}$ +, &c. be multiplied by the series $x^{\frac{1}{r}} + \frac{1}{r} z x^{\frac{1-r}{r}} + \frac{1}{r} \times \frac{1-r}{2r} z^2 x^{\frac{1-2r}{r}} + \frac{1}{r} \times \frac{1-r}{2r} \times \frac{1-2r}{3r} z^3 x^{\frac{1-3r}{r}}$ +, &c. the product will be $x^{\frac{n+1}{r}} + \frac{n+1}{r} x z^{\frac{n+1-r}{r}} + \frac{n+1}{r} \times \frac{n+1-r}{2r} z^2 x^{\frac{n+1-2r}{r}} + \frac{n+1}{r} \times \frac{n+1-r}{2r} \times \frac{n+1-2r}{3r} z^3 x^{\frac{n+1-3r}{r}}$ +, &c.

14. Upon multiplying the two last series into one another, to obtain a foundation for the demonstration in view, the same powers of x and z, which arise in the multiplication, being

placed under one another, the products will stand as below; the first two lines immediately following being considered as the multiplicand and the multiplier respectively.

$$x^{\frac{n}{r}} + \frac{n}{r}zx^{\frac{n-r}{r}} + \frac{n}{r}\times\frac{n-r}{2r}z^2x^{\frac{n-2r}{r}} + \frac{n}{r}\times\frac{n-r}{2r}\times\frac{n-2r}{3r}z^3x^{\frac{n-3r}{r}} +, \&c.$$

$$x^{\frac{1}{r}} + \frac{1}{r}zx^{\frac{1-r}{r}} + \frac{1}{r}\times\frac{1-r}{2r}z^2x^{\frac{1-2r}{r}} + \frac{1}{r}\times\frac{1-r}{2r}\times\frac{1-2r}{3r}z^3x^{\frac{1-3r}{r}} +, \&c.$$

$$x^{\frac{n+1}{r}} + \frac{n}{r}zx^{\frac{n+1-r}{r}} + \frac{n}{r}\times\frac{n-r}{2r}z^2x^{\frac{n+1-2r}{r}} + \frac{n}{r}\times\frac{n-r}{2r}\times\frac{n-2r}{3r}z^3x^{\frac{n+1-3r}{r}} +, \&c.$$

$$\frac{1}{r}zx^{\frac{n+1-r}{r}} + \frac{1}{r}\times\frac{n}{r}\,z^2x^{\frac{n+1-2r}{r}} + \frac{1}{r}\times\frac{n}{r}\times\frac{n-r}{2r}\,z^3x^{\frac{n+1-3r}{r}} +, \&c.$$

$$+\frac{1}{r}\times\frac{1-r}{2r}z^2x^{\frac{n+1-2r}{r}} + \frac{1}{r}\times\frac{1-r}{2r}\times\frac{n}{r}\,z^3x^{\frac{n+1-3r}{r}} +, \&c.$$

$$\frac{1}{r}\times\frac{1-r}{2r}\times\frac{1-2r}{3r}z^3x^{\frac{n+1-3r}{r}} +, \&c.$$

Now, in order to establish the laws of arrangement upon clear and general principles, it is necessary to observe these particulars. 1st. The exponents of the terms, both in the multiplicand and multiplier, are in arithmetical progression; they have the same denominator r, and r is also the common difference in the numerators of each progression. 2d. The multiplicand being multiplied by $x^{\frac{1}{r}}$, the first term in the multiplier, gives the first horizontal line of products; and consequently the exponents in this line are obtained from the exponents in the multiplicand by adding 1 to the numerators. The numerators, therefore, of the exponents of this line are also in arithmetical progression; and under this the other lines of products are to be arranged, so that terms which have the same exponents may come under one another. 3d. The coefficients being neglected, if any term in the mul-

tiplicand be denoted by $z^q x^{\frac{n-qr}{r}}$, the term of the multiplier immediately under will be expressed by $z^q x^{\frac{1-qr}{r}}$, according to the nature of the two series; and upon multiplying the first term of the multiplicand by this term of the multiplier, the product will be $z^q x^{\frac{n+1-qr}{r}}$, which is equal to that term of the multiplicand immediately over that in the multiplier, after 1 is added to the numerator of the exponent of x. And the other terms in the multiplicand, successively to the right hand, being multiplied by the same term of the multiplier, the terms will be $z^{q+1} x^{\frac{n+1-qr-r}{r}}$, $z^{q+2} x^{\frac{n+1-qr-2r}{r}}$, $z^{q+3} x^{\frac{n+1-qr-3r}{r}}$, &c. in arithmetical progression, which are equal to those terms of the multiplicand immediately over them, after the numerators of the exponents of x are increased by 1. And from hence a general rule is obtained for the arrangement of any horizontal line of products. For when the first term in the multiplicand is multiplied by a term in the multiplier, the product is placed immediately under that term of the multiplier; and the products which arise from multiplying the other terms of the multiplicand, successively towards the right, by the same term of the multiplier, are placed successively towards the right of the first mentioned product.

15. The several products, therefore, arranged under one another in a perpendicular line, arise in the following manner. The first arises from multiplying the term in the multiplicand directly over it into the first term in the multiplier. Thus $\frac{n}{r} \times \frac{n-r}{2r} \times \frac{n-2r}{3r} z^3 x^{\frac{n+1-3r}{r}}$ is the product of $\frac{n}{r} \times$

$\frac{n-r}{2r} \times \frac{n-2r}{3r} z^3 x^{\frac{n-3r}{r}}$, the term of the multiplicand directly over it, into $x^{\frac{1}{r}}$, the first term in the multiplier. The second term in the perpendicular line of products is obtained by multiplying that term of the multiplicand in the next perpendicular line towards the left, by the second term of the multiplier. Thus $\frac{1}{r} \times \frac{n}{r} \times \frac{n-r}{2r} z^3 x^{\frac{n+1-3r}{r}}$, is the product of $\frac{n}{r} \times \frac{n-r}{2r} z^2 x^{\frac{n-2r}{r}}$ into $\frac{1}{r} z x^{\frac{1-r}{r}}$. And in general, if p be put for a number denoting the place of a term in the perpendicular line of products, and if the terms in the multiplicand be supposed to be numbered, beginning with that directly above the perpendicular line of products under consideration, and reckoning towards the left hand; and if the terms in the line of the multiplier be numbered, beginning with $x^{\frac{1}{r}}$, and reckoning towards the right, then the product whose place is p will arise from the multiplication of that term in the multiplicand whose place is denoted by p into that term in the multiplier whose place is also denoted by p. The observations in this and the last article are evidently general; being applicable to any extent to which the series in the multiplicand and multiplier may be carried.

16. The laws of arrangement being thus established by the exponents, the summation of the coefficients, in any perpendicular line of products, is next to be attended to. And in order to do this, with as little embarrassment as possible, put $A = n$, $B = n \times \overline{n-r}$, $C = n \times \overline{n-r} \times \overline{n-2r}$, $D = n \times \overline{n-1} \times \overline{n-2r} \times \overline{n-3r}$, &c. and put $a = 1$, $b = 1 \times \overline{1-r}$, $c = 1$

$\times \overline{1-r} \times \overline{1-2r}$, $d = 1 \times \overline{1-r} \times \overline{1-2r} \times \overline{1-3r}$, &c. Moreover, put $\alpha = 1$, $\beta = 1 \times 2$, $\gamma = 1 \times 2 \times 3$, $\delta = 1 \times 2 \times 3 \times 4$, &c. and then the multiplicand, multiplier, and products will stand in the following manner, the powers of x and z being omitted.

$$1 + \frac{A}{\alpha r} + \frac{B}{\beta r^2} + \frac{C}{\gamma r^3} + \frac{D}{\delta r^4} + \frac{E}{\epsilon r^5} + \frac{F}{\zeta r^6} + \frac{G}{\eta r^7} +, \&c.$$

$$1 + \frac{a}{\alpha r} + \frac{b}{\beta r^2} + \frac{c}{\gamma r^3} + \frac{d}{\delta r^4} + \frac{e}{\epsilon r^5} + \frac{f}{\zeta r^6} + \frac{g}{\eta r^7} +, \&c.$$

$$\begin{array}{r}
1 + \frac{A}{\alpha r} + \frac{B}{\beta r^2} + \frac{C}{\gamma r^3} + \frac{D}{\delta r^4} + \frac{E}{\epsilon r^5} + \frac{F}{\zeta r^6} + \frac{G}{\eta r^7} +, \&c. \\
\frac{a}{\alpha r} + \frac{aA}{\alpha\alpha r^2} + \frac{aB}{\alpha\beta r^3} + \frac{aC}{\alpha\gamma r^4} + \frac{aD}{\alpha\delta r^5} + \frac{aE}{\alpha\epsilon r^6} + \frac{aF}{\alpha\zeta r^7} +, \&c. \\
\frac{b}{\beta r^2} + \frac{bA}{\beta\alpha r^3} + \frac{bB}{\beta\beta r^4} + \frac{bC}{\beta\gamma r^5} + \frac{bD}{\beta\delta r^6} + \frac{bE}{\beta\epsilon r^7} +, \&c. \\
\frac{c}{\gamma r^3} + \frac{cA}{\gamma\alpha r^4} + \frac{cB}{\gamma\beta r^5} + \frac{cC}{\gamma\gamma r^6} + \frac{cD}{\gamma\delta r^7} +, \&c. \\
\frac{d}{\delta r^4} + \frac{dA}{\delta\alpha r^5} + \frac{dB}{\delta\beta r^6} + \frac{dC}{\delta\gamma r^7} +, \&c. \\
\frac{e}{\epsilon r^5} + \frac{eA}{\epsilon\alpha r^6} + \frac{eB}{\epsilon\beta r^7} +, \&c. \\
\frac{f}{\zeta r^6} + \frac{fA}{\zeta\alpha r^7} +, \&c. \\
\frac{g}{\eta r^7} +, \&c.
\end{array}$$

Now the object in view, with respect to the coefficients, is to prove that the perpendicular lines of products will be, beginning at 1 and reckoning towards the right hand, equal to $1, \frac{n+1}{r}, \frac{n+1}{r} \times \frac{n+1-r}{2r}, \frac{n+1}{r} \times \frac{n+1-r}{2r} \times \frac{n+1-2r}{3r}$, &c. respectively: and this will be fully demonstrated when we have proved that all the terms of products in any perpendicular line, in which the exponent of r in the denominators is t, being

multiplied by $\frac{n+1-tr}{t+1 \times r}$ are equal to the whole of the next perpendicular line of products towards the right hand.

To do this in a manner applicable to any part of the series concerned, and to avoid numeral coefficients, which would obscure and encumber the general reasoning, it is necessary to find the value of the numerator of $\frac{n+1-tr}{t+1 \times r}$ in terms of A, B, C, D, &c. and of a, b, c, d, &c. and to ascertain the relative values of α, β, γ, δ, &c. and that we may do this with due precision and perspicuity, it is proper to fix upon two contiguous perpendicular lines of products.

17. Let the lines be those which have in their first terms F and G respectively, and then $n+1-tr = \frac{G}{F} + 1 = \frac{F}{E} + 1 - r = \frac{E}{D} + 1 - 2r = \frac{D}{C} + 1 - 3r = \frac{C}{B} + 1 - 4r = \frac{B}{A} + 1 - 5r = \frac{A}{1} + 1 - 6r$; and therefore, according to the substitution in the last article, $n+1-tr = \frac{G}{F} + a = \frac{F}{E} + \frac{b}{a} = \frac{E}{D} + \frac{c}{b} = \frac{D}{C} + \frac{d}{c} = \frac{C}{B} + \frac{e}{d} = \frac{B}{A} + \frac{f}{e} = \frac{A}{1} + \frac{g}{f}$. Now the first of the two contiguous perpendicular lines fixed upon being multiplied by these values, *viz.* the first term being multiplied by the first value, the second term by the second value, &c. and the denominators ζr^6, $\alpha\varepsilon r^6$, &c. in the first line, and ηr^7 $\alpha\zeta r^7$, &c. in the other being omitted, the two lines will be as represented in the following columns.

The first of the two contiguous lines multiplied as mentioned above.	The last of the two contiguous lines as mentioned above.
$F \times \overline{\frac{G}{F} + a} = G + aF$	G
$aE \times \overline{\frac{F}{E} + \frac{b}{a}} = aF + bE$	aF
$bD \times \overline{\frac{E}{D} + \frac{c}{b}} = bE + cD$	bE
$cC \times \overline{\frac{D}{C} + \frac{d}{c}} = cD + dC$	cD
$dB \times \overline{\frac{C}{B} + \frac{e}{d}} = dC + eB$	dC
$eA \times \overline{\frac{B}{A} + \frac{f}{e}} = eB + fA$	eB
$f \times \overline{\frac{A}{1} + \frac{g}{f}} = fA + g$	fA
	g

The proper denominators being annexed to these terms, and v being put for $t + 1$, it now remains to be proved that

$$\frac{G+aF}{\zeta r^6 \times vr} + \frac{aF+bE}{\alpha\varepsilon r^6 \times vr} + \frac{bE+cD}{\beta\delta r^6 \times vr} + \frac{cD+dC}{\gamma\gamma r^6 \times vr} + \frac{dC+eB}{\delta\beta r^6 \times vr} + \frac{eB+fA}{\varepsilon\alpha r^6 \times vr} + \frac{fA+g}{\zeta r^6 \times vr} = \frac{G}{\eta r^7} + \frac{aF}{\alpha\zeta r^7} + \frac{bE}{\beta\varepsilon r^7} + \frac{cD}{\gamma\delta r^7} + \frac{dC}{\delta\gamma r^7} + \frac{eB}{\varepsilon\beta r^7} + \frac{fA}{\zeta\alpha r^7} + \frac{g}{\eta r^7}.$$

18. The relative values, therefore, of α, β, γ, &c. next claim our attention; and from the nature of the series, $\frac{\eta}{v} = \zeta$, $\frac{\zeta}{v-1} = \varepsilon$, $\frac{\varepsilon}{v-2} = \delta$, $\frac{\delta}{v-3} = \gamma$, $\frac{\gamma}{v-4} = \beta$, $\frac{\beta}{v-5} = \alpha$, $\frac{\alpha}{v-6} = 1$. Also $1 = \alpha$, $\frac{\beta}{2} = \alpha$, $\frac{\gamma}{3} = \beta$, $\frac{\delta}{4} = \gamma$, $\frac{\varepsilon}{5} = \delta$, $\frac{\zeta}{6} = \varepsilon$, $\frac{\eta}{7} = \zeta$. As the powers of r in the equation, asserted in the end of the last article, are the same in all the terms, they may be neglected; the only thing necessary is to reduce ζ, $\alpha\varepsilon$, $\beta\delta$, $\gamma\gamma$, $\delta\beta$, $\varepsilon\alpha$, ζ, the denominators of the first side, to η, $\alpha\zeta$, $\beta\varepsilon$, $\gamma\delta$, $\delta\gamma$, $\varepsilon\beta$, $\zeta\alpha$, η, the de-

nominators of the second, and in such a manner as to make the parts on the first side, which have the same numerators, unite: thus the part $\frac{G}{\zeta}$ must be reduced to the denominator η; the parts $\frac{aF}{\zeta v} + \frac{aF}{\alpha\iota v}$ must be reduced to the same denominator $\alpha\zeta$; the parts $\frac{bE}{\alpha\iota v} + \frac{bE}{\beta\delta v}$ to the same denominators $\beta\epsilon$, &c.

First line.	Second line.
$\frac{G+aF}{\zeta v}$	$\frac{G}{\eta}$
$\frac{aF+bE}{\alpha\iota v}$	$\frac{aF}{\alpha\zeta}$
$\frac{bE+cD}{\beta\delta v}$	$\frac{bE}{\beta\epsilon}$
$\frac{cD+dC}{\gamma\gamma v}$	$\frac{cD}{\gamma\delta}$
$\frac{dC+eB}{\delta\beta v}$	$\frac{dC}{\delta\gamma}$
$\frac{eB+fA}{\iota\alpha v}$	$\frac{eB}{\iota\beta}$
$\frac{fA+g}{\zeta v}$	$\frac{fA}{\zeta\alpha}$
	$\frac{g}{\eta}$

Now, upon examining the two lines as represented in the columns in the margin, a general rule for this reduction presents itself. For the denominators, exclusive of v in the first column, proceed in the following regular manner, which is not peculiar to the perpendicular lines now under examination, but is the same in any two contiguous lines in any period of the multiplication exhibited in the 16th article. The first and last denominator, in each column, consists of a single letter, as ζ in the first, and η in the second, of these we have selected for illustration. The second denominator consists of the next lower letter to the highest multiplied into α, as $\alpha\epsilon$ in the first column, and $\alpha\zeta$ in the second. The third denominator consists of the second lower letter to the highest multiplied into β, as $\beta\delta$ in the first column, and $\beta\epsilon$ in the second; and the same gradation is observed in the other denominators. Now each term in the first column has two members in the numerator, and to make these unite with the terms in the second column, the first member must have the same denominator with that term in the second column in the same horizontal line; and the second member must have the

same denominator with that term in the second column on the next lower horizontal line. For the first member, therefore, the second letter in the denominator must be raised to the next higher letter by substitution; and for the second member the first letter in the denominator must be raised to the next higher by substitution. For each denominator, therefore, in the first column two equal values must be found accordingly, and the first value must be put under the first member, and the second value under the second member of the numerators. Hence the values for the denominators of the first line will be obtained in the following manner, from the equations in the beginning of this article. For the first term $\zeta v = \frac{\eta}{v} \times v = \eta = \alpha \zeta v$, for $\alpha = 1$; for the second term $\alpha \varepsilon v = \alpha \times \frac{\zeta}{v-1} \times v = \frac{\alpha \zeta v}{v-1} = \frac{\beta}{2} \times \varepsilon v = \frac{\beta \varepsilon v}{2}$; for the third, $\beta \delta v = \beta \times \frac{\varepsilon}{v-2} \times v = \frac{\beta \varepsilon v}{v-2} = \frac{\gamma}{3} \times \delta v = \frac{\gamma \delta v}{3}$; for the fourth, $\gamma \gamma v = \gamma \times \frac{\delta}{v-3} \times v = \frac{\gamma \delta v}{v-3} = \frac{\delta}{4} \times \gamma v = \frac{\delta \gamma v}{4}$; for the fifth, $\delta \beta v = \delta \times \frac{\gamma}{v-4} \times v = \frac{\delta \gamma v}{v-4} = \frac{\varepsilon}{5} \times \beta v = \frac{\varepsilon \beta v}{5}$; for the sixth, $\varepsilon \alpha v = \varepsilon \times \frac{\beta}{v-5} \times v = \frac{\varepsilon \beta v}{v-5} = \frac{\zeta}{6} \times \alpha v = \frac{\zeta \alpha v}{6}$; for the last, $\zeta v = \zeta \times \frac{\alpha}{v-6} \times v = \frac{\zeta \alpha v}{v-6} = \frac{\eta}{7} \times v = \eta$. It therefore follows that

$$\frac{G+aF}{\zeta v}=\frac{G}{\eta}+\frac{aF}{\alpha\zeta v}$$

$$\frac{aF+bE}{\alpha\iota v}=\cdots\frac{aF\times\overline{v-1}}{\alpha\zeta v}+\frac{bE\times 2}{\beta\iota v}$$

$$\frac{bE+cD}{\beta\delta v}=\cdots\cdots\frac{bE\times\overline{v-2}}{\beta\iota v}+\frac{cD\times 3}{\gamma\delta v}$$

$$\frac{cD+dC}{\gamma\gamma v}=\cdots\cdots\cdots\frac{cD\times\overline{v-3}}{\gamma\delta v}+\frac{dC\times 4}{\delta\gamma v}$$

$$\frac{dC+eB}{\delta\beta v}=\cdots\cdots\cdots\cdots\frac{dC\times\overline{v-4}}{\delta\gamma v}+\frac{eB\times 5}{\iota\beta v}$$

$$\frac{eB+fA}{\iota\alpha v}=\cdots\cdots\cdots\cdots\cdots\frac{eB\times\overline{v-5}}{\iota\beta v}+\frac{fA\times 6}{\zeta\alpha v}$$

$$\frac{fA+g}{\zeta v}=\cdots\cdots\cdots\cdots\cdots\cdots\frac{fA\times\overline{v-6}}{\zeta\alpha v}+\frac{g}{\eta}$$

And consequently $\frac{G+aF}{\zeta v}+\frac{aF+eB}{\alpha\iota v}+\frac{bE+cD}{\beta\delta v}+\frac{cD+dC}{\gamma\gamma v}+\frac{dC+eB}{\delta\beta v}+\frac{eB+fA}{\iota\alpha v}+\frac{fA+g}{\zeta v}=\frac{G}{\eta}+\frac{aF}{\alpha\zeta}+\frac{bE}{\beta\iota}+\frac{cD}{\gamma\delta}+\frac{dC}{\delta\gamma}+\frac{eB}{\iota\beta}+\frac{fA}{\zeta\alpha}+\frac{g}{\eta}$.

19. This being proved from the relations between the two contiguous perpendicular lines, and these relations being the same between any two perpendicular lines whatever (for they are as regular and certain as the laws of continuation in the multiplicand and multiplier with which we set out in the 13th article) it follows that if $\frac{pz^m x^{\frac{\overline{n+1-mr}}{r}}}{qr^m}$ express the whole of any perpendicular line, the next perpendicular line to the right will be $\frac{p\times\overline{n+1-mr}\,z^{m+1}x^{\frac{n+1-\overline{m+1}\,r}{r}}}{q\times\overline{m+1}\,r^{m+1}}$. And therefore the series $x^{\frac{n}{r}}+\frac{n}{r}zx^{\frac{n}{r}-1}+\frac{n}{r}\times\frac{\frac{n}{r}-1}{2}z^2x^{\frac{n}{r}-2}+\frac{n}{r}\times\frac{\frac{n}{r}-1}{2}\times$

$\frac{\frac{n}{r}-2}{3} z^3 x^{\frac{n}{r}-3}$ +, &c. being multiplied by the series $x^{\frac{1}{r}}$ + $\frac{1}{r} z x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} z^2 x^{\frac{1}{r}-2} + \frac{1}{r} \times \frac{\frac{1}{r}-2}{2} \times \frac{\frac{1}{r}-1}{3} z^3 x^{\frac{1}{r}-3}$ +, &c. the product will be $x^{\frac{n+1}{r}} + \frac{n+1}{r} z x^{\frac{n+1}{r}-1} + \frac{n+1}{r} \times$ $\frac{\frac{n+1}{r}-1}{2} z^2 x^{\frac{n+1}{r}-2} + \frac{n+1}{r} \times \frac{\frac{n+1}{r}-1}{2} \times \frac{\frac{n+1}{r}-2}{3} z^3 x^{\frac{n+1}{r}-3}$ +, &c.

20. From hence it follows, that $\overline{x+z}^{\frac{n}{r}} = x^{\frac{n}{r}} + \frac{n}{r} z x^{\frac{n}{r}-1}$ $+ \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} \times \frac{\frac{n}{r}-2}{3} z^3 x^{\frac{n}{r}-3}$ +, &c. For as n in the last article may denote any number whatever, the square of the series $x^{\frac{1}{r}} + \frac{1}{r} z x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} z^2 x^{\frac{1}{r}-2} + \frac{1}{r}$ $\times \frac{\frac{1}{r}-1}{2} \times \frac{\frac{1}{r}-2}{3} z^3 x^{\frac{1}{r}-3}$ +, &c. will be $x^{\frac{2}{r}} + \frac{2}{r} z x^{\frac{2}{r}-1} + \frac{2}{r}$ $\times \frac{\frac{2}{r}-1}{2} z^2 x^{\frac{2}{r}-2} + \frac{2}{r} \times \frac{\frac{2}{r}-1}{2} \times \frac{\frac{2}{r}-2}{3} z^3 x^{\frac{2}{r}-3}$ +, &c. and this being multiplied by $x^{\frac{1}{r}} + \frac{1}{r} z x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} z^2 x^{\frac{1}{r}-2}$ + $\frac{1}{r} \times \frac{\frac{1}{r}-1}{2} \times \frac{\frac{1}{r}-2}{3} z^3 x^{\frac{1}{r}-3}$ +, &c. the product will be $x^{\frac{3}{r}}$ + $\frac{3}{r} z x^{\frac{3}{r}-1} + \frac{3}{r} \times \frac{\frac{3}{r}-1}{2} z^2 x^{\frac{3}{r}-2} + \frac{3}{r} \times \frac{\frac{3}{r}-1}{2} \times \frac{\frac{3}{r}-2}{3} z^3 x^{\frac{3}{r}-3}$ +, &c. 1 being added to the numerator of the fraction of which r is the denominator, upon every multiplication. The

nth power, therefore, of the series $x^{\frac{1}{r}} + \frac{1}{r} z x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} z^2 x^{\frac{1}{r}-2} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} \times \frac{\frac{1}{r}-2}{3} z^3 x^{\frac{1}{r}-3} +$, &c. is equal to $x^{\frac{n}{r}} + \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} \times \frac{\frac{n}{r}-2}{3} z^3 x^{\frac{n}{r}-3} +$, &c. and this series, when n is equal to r, becomes equal to $x+z$. Hence $\overline{x+z}^{\frac{1}{r}} = x^{\frac{1}{r}} + \frac{1}{r} z x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} z^2 x^{\frac{1}{r}-2} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} \times \frac{\frac{1}{r}-2}{3} z^3 x^{\frac{1}{r}-3} +$, &c. and consequently $\overline{x+z}^{\frac{n}{r}} = x^{\frac{n}{r}} + \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} \times \frac{\frac{n}{r}-2}{3} z^3 x^{\frac{n}{r}-3} +$, &c.

21. From the preceding method of investigating the theorem it also follows that $\overline{x-z}^{\frac{n}{r}} = x^{\frac{n}{r}} - \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} - \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} \times \frac{\frac{n}{r}-2}{3} z^3 x^{\frac{n}{r}-3} +$, &c. For the series $x^{\frac{n}{r}} - \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} - \frac{n}{r} \times \frac{\frac{n}{r}-1}{2} \times \frac{\frac{n}{r}-2}{3} z^3 x^{\frac{n}{r}-3} +$, &c. being multiplied by $x^{\frac{1}{r}} - \frac{1}{r} z x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} z^2 x^{\frac{1}{r}-2} - \frac{1}{r} \times \frac{\frac{1}{r}-1}{2} \times \frac{\frac{1}{r}-2}{3} z^3 x^{\frac{1}{r}-3} +$, &c. the product will stand as below, the laws of arrangement being the same as those established in the 14th and 15th articles.

$$x^{\frac{n}{r}} - \frac{n}{r} z x^{\frac{n-r}{r}} + \frac{n}{r}\times\frac{n-r}{2r} z^2 x^{\frac{n-2r}{r}} - \frac{n}{r}\times\frac{n-r}{2r}\times\frac{n-2r}{3r} z^3 x^{\frac{n-3r}{r}} +, \&c.$$

$$x^{\frac{1}{r}} - \frac{1}{r} z x^{\frac{1-r}{r}} + \frac{1}{r}\times\frac{1-r}{2r} z^2 x^{\frac{1-2r}{r}} - \frac{1}{r}\times\frac{1-r}{2r}\times\frac{1-2r}{3r} z^3 x^{\frac{1-3r}{r}} +, \&c.$$

$$x^{\frac{n+1}{r}} - \frac{1}{r} z x^{\frac{n+1-r}{r}} + \frac{n}{r}\times\frac{n-r}{2r} z^2 x^{\frac{n+1-2r}{r}} - \frac{n}{r}\times\frac{n-r}{2r}\times\frac{n-2r}{3r} z^3 x^{\frac{n+1-3r}{r}} +, \&c.$$

$$- \frac{1}{r} z x^{\frac{n+1-r}{r}} + \frac{1}{r}\times\frac{n}{r} z^2 x^{\frac{n+1-2r}{r}} - \frac{1}{r}\times\frac{n}{r}\times\frac{n-r}{2r} z^3 x^{\frac{n+1-3r}{r}} +, \&c.$$

$$\frac{1}{r}\times\frac{1-r}{2r} z^2 x^{\frac{n+1-2r}{r}} - \frac{1}{r}\times\frac{1-r}{2r}\times\frac{n}{r} z^3 x^{\frac{n+1-3r}{r}} +, \&c.$$

$$- \frac{1}{r}\times\frac{1-r}{2r}\times\frac{1-2r}{3r} z^3 x^{\frac{n+1-3r}{r}} +, \&c.$$

And from hence it is evident that these perpendicular lines differ from those in the 14th article, in the signs only; the signs in the above being alternately $+$ and $-$. It therefore may be demonstrated, as in the foregoing articles, that $\overline{x-z}|^{\frac{n}{r}}$

$$= x^{\frac{n}{r}} - \frac{n}{r} z x^{\frac{n}{r}-1} + \frac{n}{r}\times\frac{\frac{n}{r}-1}{2} z^2 x^{\frac{n}{r}-2} - \frac{n}{r}\times\frac{\frac{n}{r}-1}{2}\times\frac{\frac{n}{r}-2}{3}$$

$$z^3 x^{\frac{n}{r}-3} +, \&c.$$

XVII. *Experiments and Observations to investigate the Nature of a Kind of Steel, manufactured at* Bombay, *and there called* Wootz*: with Remarks on the Properties and Composition of the different States of Iron. By* George Pearson, *M. D. F. R. S.*

Read June 11, 1795.

§ 1.

DOCTOR SCOTT, of Bombay, in a letter to the President, acquaints him that he has sent over specimens of a substance known by the name of wootz; which is considered to be a kind of steel, and is in high esteem among the Indians. Dr. SCOTT mentions several of its properties, and requests that an inquiry may be instituted to obtain further knowledge of its nature. This gentleman informs the President, that wootz "admits of a harder temper than any thing known in that "part of India; that it is employed for covering that part of "gun-locks which the flint strikes: that it is used for cut-"ting iron on a lathe; for cutting stones; for chizzels; for "making files; for saws; and for every purpose where exces-"sive hardness is necessary." Dr. SCOTT observes that this substance "cannot bear any thing beyond a very slight red heat, "which makes it work very tediously in the hands of smiths;" and that "it has a still greater inconvenience or defect, that "of not being capable of being welded with iron or steel; "to which therefore it is only joined by screws and other

"contrivances." He likewise observes, that when wootz is "heated above a slight red heat, part of the mass seems to "run, and the whole is lost, as if it consisted of metals of different degrees of fusibility." We learn also from Dr. SCOTT's letter, that "the working with wootz is so difficult, that it is a "separate art from that of forging iron." It will be proper also to notice his observation, that "the magnetical power in "an imperfect degree can be communicated to this substance."

§ 2. *Mechanical and obvious Properties.*

The specimens of wootz were in the shape of a round cake, of about five inches in diameter, and one thick; each of which weighed somewhat more than two pounds. The cake had been cut almost quite through, so as to nearly divide it into two equal parts. It was externally of a dull black colour; the surface was smooth; the cut part was also smooth, and, excepting a few *pinny* places and small holes, the texture appeared to be uniform. It felt about as heavy as an equal bulk of iron or steel. It was tasteless and inodorous. No indentation could be made by blows with a heavy hammer; nor was it broken by blows which I think would have broken a like piece of our steel. Fire was elicited on collision with flint. Under the file I found wootz much harder than common bar steel not yet hardened, and than HUNTSMAN's cast steel not yet hardened. It seemed to possess the hardness of some kinds of crude iron, but did not effectually resist the file like highly tempered steel, and many sorts of crude iron: for although the teeth of the file were rapidly worn down and broken, the wootz was also reduced to the state of filings. The filed surface was of a bright bluish colour, shining like hardened steel; but some parts were brighter than others; and the most shin-

ing places seemed to be the hardest parts: hence perhaps the reason of the surface being uneven, and a little *pinny*. Notwithstanding this uneven and *pinny* appearance of the filed surface, a polish was produced, which was I think at least equal, if not superior, in brilliancy and smoothness to that of any steel I ever saw. The wootz filings were attracted by the magnet like common iron filings.

A cake of this substance being broken in the part nearly cut through, the fracture exhibited the grain and colour of rather open grained steel, but it was not nearly so open as I have constantly seen the grain of a bar of cement, or blister steel. The grain of wootz was most like that of blister steel which has been heated and hammered a little, and also like some kinds of refined crude iron.

The specific gravity of wootz, and several specimens of steel and iron, was found, by Mr. MORE and myself, to be as follows.

No. 1. Wootz - - - -	7.181
No. 2. Another specimen of wootz -	7.403
No. 3. Ditto forged - - -	7.647
No. 4. Another specimen, forged - -	7.503
No. 5. Wootz which had been melted -	7.200
No. 6. Wootz which had been quenched while white hot - - - - -	7.166
No. 7. Bar steel from Oeregrund iron -	7.313
No. 8. Ditto hammered - - -	7.735
No. 9. German steel bar, said to be directly from the ore	7.500
No. 10. Ditto quenched when white hot -	7.370
No. 11. Melted steel wire - - -	7.500
No. 12. Ditto, another parcel - -	7.460
No. 13. Piece of hammered Oeregrund steel bar after quenching when white hot - - -	7.555

No. 14.	Another parcel of ditto - -	7.570
No. 15.	Piece of same bar hammered, but not hardened by quenching - - - -	7.693
No. 16.	Piece of steel which had been often heated and cooled gradually - - - -	7.308
No. 17.	HUNTSMAN's steel hammered -	7.916
No. 18.	Ditto, another specimen - -	7.826
No. 19.	Ditto, another specimen - -	7.830
No. 20.	Ditto quenched when white hot -	7.771
No. 21.	Ditto, another specimen so quenched	7.765
No. 22.	Piece of a file quenched while white hot to produce the appearance called, *open grain* - -	7.352
No. 23.	Another specimen of ditto - -	7.405
No. 24.	Piece of same file, but not so quenched	7.460
No. 25.	Another specimen of ditto -	7.585
No. 26.	Piece of very hard steel - -	7.260
No. 27.	Hammered common steel - -	7.794
No. 28.	Another specimen of ditto, and hardened by quenching - - - -	7.676
No. 29.	Softest and toughest hammered iron; from PARKES, an iron merchant - - -	7.716
No. 30.	Another specimen of ditto -	7.700
No. 31.	Another parcel of ditto - -	7.780
No. 32.	Another specimen of ditto - -	7.787
No. 33.	Common hammered iron - -	7.600
No. 34.	Another specimen of ditto -	7.450
No. 35.	Cast or brittle iron re-melted * -	7.012

* BERGMAN states the specific gravities of steel and iron as follows: 1, steel 7,643.—2, Ditto 7,775.—3, Ditto 7,727.—4, Ditto 7,784.—5, Ditto indurated 7,693.—6, Wrought iron 7,798.—7, Ditto 7,829.

§ 3. *Effects of Fire.*

Until the substance was made red hot I could not scarcely make any impression with a hammer; nor could it be cut through by a chizzel, or wedge, till it was ignited to be of a pale red colour. It had then the peculiar smell of iron: it was then malleable, but was much more liable to be cracked and fractured by the hammer than common steel; or than, I think, even cast steel. Small and thin pieces are perhaps malleable at lower degrees of fire, but very slowly, and not without great care and management. That ingenious artist, Mr. STODART, forged a piece of wootz, at the desire of the President, for a penknife, at the temperature of ignition in the dark. It received the requisite temper.* The edge was as fine, and cut as well as the best steel knife. Notwithstanding the difficulty and labour in forging, Mr. STODART from this trial was of opinion, that wootz is superior for many purposes to any steel used in this country. He thought it would carry a finer, stronger, and more durable edge, and point. Hence it might be particularly valuable for lancets, and other chirurgical instruments.

Mr. MORE got a piece of wootz beat into a thin plate: in this state the texture did not seem to be uniform, but appeared to be of different degrees of hardness or kinds. A large piece also was forged into a thick bar for Sir THOMAS FRANKLAND.

(*a*) The pieces which had been cut in the ignited state abovementioned had smooth surfaces, with a few small cavities.

(*b*) The substance made white hot, by the forge, had the glassy smooth surface of iron, in what is termed the weld-

* "At the temperature of 450° of FAHRENHEIT's scale."—Mr. STODART's letter to the President.

ing, or the welling* state. On striking it gently under the hammer, it was cracked in many places: and by a hard blow it was broken into a number of small pieces, as crude iron and cast steel are at this degree of ignition.

(*c*) The surfaces of the fractured pieces (§ 3. *b*) were black and ragged, or, as it is termed, had no grain. Two or three pieces indeed had yellow and reddish spots; but these were merely tinges from the fire, and disappeared on applying a few drops of muriatic acid.

(*d*) The pieces (§ 3. *c*) when cold were readily broken. Some of the fractures exhibited a bright silvery foliated grain, of seemingly an homogeneous substance, as frequently appears on breaking steel which has been quenched, when white hot, in cold water; and as also appears on breaking steel and crude iron which have been repeatedly ignited and cooled gradually; but many of the fractures of the small pieces were gray and close grained.

(*e*) A piece of the substance was ignited to whiteness, and then quenched in a large bulk of cold water. It was rendered much harder than before, so that a good file rubbed off very little. I cannot however from this experiment determine whether wootz is susceptible of a greater, or so great a degree of hardness as some kinds of steel used by the English artists.

(*f*) The piece (§ 3. *e*) was ignited in a close vessel, and let cool in the ashes of the fuel. It became much less hard, but I never could by annealing bring down the temper to the degree of any of our steels: on which account it is far more difficult to forge. The interior parts of a thick piece of wootz could not scarcely be softened at all by annealing.

* This term being from the German word *wellen*.

(*g*) A piece of the substance, about 500 grains in weight (wrapped in paper to afford carbon enough to prevent oxidation, without supersaturating the metal with carbon) was exposed in a close vessel for above an hour to a pretty considerable fire. On cooling, the substance was found to have retained its form, but it was of a slate-blue colour, and many round particles as large as pins heads adhered to its surface, as if matter had oozed out by melting. The degree of fire, indicated by WEDGWOOD's pyrometer, was 140°.

A piece of our steel, which had been a part of a file, was exposed in a similar manner, but to rather more fire. It retained its form, and its surface remained smooth.

A piece of crude, or cast iron, by exposure to this degree of fire, under the circumstances just mentioned, was fused: but in a temperature of about 120° its surface became covered with a number of smooth roundish masses, as if fusion had begun.

(*h*) 500 grains of wootz were exposed as in the former experiment, but to a fiercer fire, in my forge. The temperature was 148°; which is 23° more than Mr. WEDGWOOD states he could produce in a common smith's forge. My forge is moveable: the fuel is contained in a pan of cast iron lined with fire-bricks, as proposed by Mr. MORE: the bellows are only of the 22 inch size. In this fire the substance was melted with the loss of a few grains in weight. The surface was quite smooth. It broke under the hammer like cast steel. It received as fine a polish as that which had not been melted. Under a lens the polished surface appeared quite uniform and close, with a few pores at equal distances. The polished unmelted wootz had still fewer pores, and at unequal distances, but with several fissures. Its grain, in the opinion of Mr.

Stodart, was like that of cast steel of the best quality; consequently it was uniform and rather close. Its specific gravity, as already stated, was about 7,200.

500 grains of steel wire melted under the circumstances just mentioned. The mass which had been fused was fractured in the same manner, and had the same kind of grain, as wootz which had been melted.

I did not always succeed in melting wootz and steel, although the fire denoted by the pyrometer was of the same, or a higher temperature than that in which at other times they were melted. Nor is this result difficult to account for by those who consider the different temperatures in different parts of the same fire; even supposing the instrument to invariably indicate the real temperature.

(*i*) Equal weights, namely, 500 grains of wootz, steel wire, and gray pig iron, were exposed, for half an hour, in the same crucible well covered, to a pretty considerable fire. On cooling, the pig iron was found to have been fused, but the other two states of iron had retained their form. The pyrometer was contracted to near the 140th degree.

(*k*) I melted together 500 grains of steel wire and 50 grains of gray pig iron, in a close vessel, without any addition of carbon. The steel so alloyed was more brittle than cast steel. Its grain was coarser, and it had not the uniformity of texture and colour of melted wootz (§ 3. *h*); but had more resemblance to some of the fractures of the unmelted wootz (§ 3. *d*).

§ 4. *Effects of Fire and Oxygen Gaz conjointly.*

A piece of wootz ignited to whiteness, being exposed to a

blast of air in the charcoal fire of the forge, emitted sparks like those of iron, and steel, in these circumstances. At the same time it melted in the state of oxide of iron.

§ 5. *Experiments with diluted Nitrous Acid.*

(*a*) 200 grains of the substance under examination were first digested, and afterwards boiled in three ounce measures of concentrated nitrous acid mixed with an equal bulk of water. A dissolution took place, with a discharge of nitrous gaz. The mixture, reduced by boiling to half its bulk, was diluted with water, and while boiling hot was filtrated through paper. Excepting a few grains of black matter, the whole mixture passed through the filtre. The filtrated liquor evaporated to dryness afforded matter, which after being kept red hot for two hours was a light spongy reddish substance; that weighed 270 grains.

(*b*) 30 grains of the reddish substance (§ 5. *a*) digested in half an ounce of concentrated acetic acid, on filtration and evaporation to dryness yielded one grain and a half of gray matter, which was ascertained to be oxide of iron.

(*c*) The blackish matter left upon the filtre (§ 5. *a*) was repeatedly digested in diluted nitrous acid. The filtrated liquors on evaporation afforded at first a few grains of oxide of iron, and at last a very minute quantity.

(*d*) 60 grains of the reddish matter (§ 5. *a*) with a bit of sugar, were digested in diluted nitrous acid. The filtrated liquid on evaporation to dryness yielded a few grains of a brownish substance, which after many experiments, was found to be oxide of iron. Of these it will be satisfactory if I men-

tion, that a little of the brownish substance fused with the fluxes by the flame and blowpipe, did not afford a reddish or purple glass from the exterior or white flame; nor a colourless one from the interior blue flame.

The experiments (§ 5. *a*—*d*) were also made on steel wire with the same result.

(*e*) A few drops of diluted nitrous acid were applied to a piece of polished wootz, steel, and iron. The parts of the wootz and steel so wetted became black, but the iron was made brown.

§ 6. *Experiments with diluted Sulphuric Acid.*

This acid liquor was made by mixing one measure of concentrated sulphuric acid with three of pure water.

Before I felt any degree of confidence in these experiments with respect to the carbon, and the proportions of hydrogen gaz from wootz and water, I repeated them often; but I here think it necessary to relate only one experiment.

200 grains of wootz, from the surface of which oxide, and any other extraneous matter, had been carefully rubbed off, were put into a retort with five ounce measures of diluted sulphuric acid. In the temperature of 55° of the room, in twenty-four hours, about a pint measure of gaz came over into a jar filled with, and standing over, lime-water; without any disturbance of its transparency, or diminution of the bulk of the gaz. The liquid in the retort became green, and a quantity of black wool-like sediment appeared upon the undissolved wootz.

On applying the lamp the dissolution went on rapidly, and black matter continued to be separated, and gaz to rise, till the whole of what seemed to be soluble in the menstruum

disappeared. When about three-fourths of the matter were dissolved, a white sediment like the *siderite* of BERGMAN began to appear, and increased as the dissolution went on.

By standing, still more of this white sediment fell down, and green crystals, apparently those of sulfate of iron, formed a stratum which lay over the white matter. The black matter adhered to the sides of the retort, it appeared also upon the surface of the liquid, and some of it was deposited under the white sediment.

This experiment was made with steel wire, and the toughest iron wire.

The phænomena during the dissolution of steel were the same as those last related; except such as obviously arose from mechanical differences in the substances to be dissolved. In particular the quantity of insoluble black matter, of white sediment, and of green crystals, was apparently the same. But with respect to the phænomena of the dissolution of iron, there was one material difference between it and the dissolution of wootz and steel, namely, that the liquor was not turbid and black, but clear, with a very small quantity of black matter upon its surface. It is however proper to state, that seemingly the same kind and quantity of white sediment and green crystals were produced as from the dissolution of wootz and steel.

I think it of consequence also to notice, that the black matter appears in the greatest quantity when about half, or three-fourths of the matter is dissolved; but after this period, although gaz be separated in as great quantity as before, the black matter seems to diminish. Hence I was at first inclined to conclude with Mr. BERTHOLLET, that part of this black or carbonaceous matter was dissolved by the gaz, but I think I

shall prove, that no such combination takes place; and I now consider it to be most probable, that the diminution arises from the dissolution of the last portions of adhering iron.

With respect to the quantities and nature of the gaz separated in this experiment:

I. The quantity of it from each hundred grains of wootz, on trials at different times, was found to be from 78 to 84 ounce measures: the mean quantity was therefore 81.

II. The gaz from each hundred grains of steel wire, after many trials, was found to be from 83 to 86 ounce measures: the mean quantity was therefore $84\frac{1}{2}$.

III. The gaz from each hundred grains of bright iron wire, by many trials with the same and different parcels of wire, was found to be from 86 to 88 ounce measures: the mean quantity therefore was 87.

It is to be understood, that when the quantities of the different parcels of gaz were compared with one another they were measured at the same temperature, and under the same degree of pressure. It is likewise to be understood, that whenever the solutions of wootz, steel, and iron, were made at the same time, and under the same circumstances as far as known, there was uniformly a smaller bulk of gaz from wootz than from steel, and from steel than from iron.

The smell of the gaz from the above three substances was that of hydrogen gaz: but I thought that from wootz had a stronger and more offensive smell than from steel; and that from steel was more offensive than from iron.

I could perceive no difference in the kind of flame, and explosion, between these three parcels of hydrogen gaz: they

burned in the same manner as common hydrogen gaz from sulphuric acid, iron, and water.

Portions of the above gazes mixed with oxygen gaz, from oxide of manganese, were burned in close vessels by the electric fire, over lime-water. I could perceive no difference in the combustion between the gazes from the above different substances, nor any difference in the gaz from the same substance at different stages of the dissolution. I did not perceive the lime-water to be at all disturbed in its transparency on my first trials; but in subsequent ones, on viewing it more attentively, and in a good light, it was perceived to be very slightly turbid. It was equally so with all the parcels of gaz.

To satisfy myself further, at the time I made these experiments I exploded the mixture of inflammable gaz, obtained by decompounding water with white hot charcoal of wood, with oxygen gaz; by which the lime-water was rendered quite milky. This inflammable gaz burnt very slowly, affording a deep blue lambent flame.

To determine the quantity, and ascertain the nature, of the undissolved black matter in this experiment, I poured the solutions, while boiling hot, upon filtres of three folds of paper, and freed the filtres from the adhering solutions by pouring boiling water upon them. The paper was stained black by the solutions of wootz and steel, as far as the liquid reached, but the paper was only stained black at the apex of the cone of the filtre by the solution of iron. The quantity of black matter on the filtres from the two former solutions was apparently six or eight times more than from the solution of iron: but it adhered too firmly, and was in too small a quantity, to determine the proportion accurately by weight. I estimated the quantity

of the black matter to be one *per cent.* of the steel and wootz, and a proportionally smaller quantity from the iron. On account of the very black and turbid appearance during the dissolution of wootz and steel, I was much surprised by the smallness of the quantity of black matter on the filtres; nor could I by experiment find that any of it passed through the filtres with the solutions.

This black matter being sprinkled upon boiling nitre, a deflagration took place, and a large proportion of residue was found, and ascertained to be oxide of iron. The black matter was therefore a compound of iron and carbon, or, as some chemists term it, plumbago; and which in the new system is denominated a carburet of iron.

I estimate the quantity of carbon in wootz and steel to be nearly equal; and that quantity to be about one-third of a hundredth part, or $\frac{1}{300}$ of the weight of these two substances.

I am in the next place to give an account of the solutions just mentioned of wootz, steel, and iron. On standing, it has been observed, there was a deposition of white matter, and formation of green crystals in a liquid.

The liquid being decanted, was examined, and found to be sulfate of iron and superabundant diluted sulphuric acid.

The green crystals were obviously those of sulfate of iron.

The white matter I supposed was the *siderite* of BERGMAN; which is now believed to be phosphate of iron. I made many experiments to ascertain its nature, but it is only necessary to state; that it readily dissolved in hot water, and the solution afforded nothing but crystals of sulfate of iron. These crystals, by dissolving in a little water, and by boiling to leave behind

water insufficient for crystallization, yielded on cooling a white sediment as before.

This white matter yielded colcothar, a red oxide of iron, by applying the flame with the blowpipe. The white matter therefore was not *siderite* but sulfate of iron, which could not crystallize on account of deficiency of water.

§ 7. *Experiments with Oxide of Wootz, Steel, and Iron.*

1200 grains of wootz dissolved by diluted sulphuric acid, and then precipitated from this acid by pot-ash, yielded greenish oxide; which on drying in a stove became a reddish-brown light powder, weighing 2700 grains; and by ignition it was reduced to 2000 grains.

300 grains of this oxide were made into a paste with linseed oil; which, being wrapped in paper, was put into a crucible and exposed for near an hour to a fierce fire in the wind furnace. On cooling, a cake of metal weighing 200 grains was obtained, which had the essential properties of steel. The pyrometer denoted 150° of fire.

The result was the same on treating oxide of steel, and of iron, in the same manner as wootz.

CONCLUSIONS.

Many of the properties of wootz, related in the preceding experiments and observations, are so generally known to be those of the metallic state of iron that, but for the sake of order, I should think it superfluous to refer to any of them particularly, to support the conclusion that wootz is at least

principally iron. Wootz is proved to be iron by the obvious properties (§ 2.); by its filings being attracted by the magnet (§ 2.); by its specific gravity (§ 2.); by its affording nothing but sulfate of iron, hydrogen gaz, and a trifling residue, on solution in diluted sulphuric acid (§ 6.).

With regard to the particular state of iron, called wootz, I think I cannot explain its nature satisfactorily, without first relating the properties, and explaining the interior structure, of the principal different metallic states of iron. I imagine I shall be best able to execute this design by stating precisely the just meaning of the terms, which denote, commonly, the three principal metallic states of iron, namely, *wrought or forged iron*, *steel*, and *cast or raw iron*.

1. *Wrought* or *forged iron* I understand to be that which possesses the following properties.

a. It is malleable and ductile in every temperature; and the more readily the higher the temperature.

b. It is susceptible of but little induration (and if pure it is most probably susceptible of none at all) by immersing it, when ignited, in a cold medium; as in water, fat oil, mercury. Nor is it on the contrary susceptible of emollition by igniting, and letting the fire be separated from it very gradually.

c. It cannot be melted, without addition; but it may be rendered quite soft by fire, and in that soft state it is very tough and malleable.

d. It can easily be reduced to filings.

e. By being surrounded with carbon for a sufficient length of time, at a due temperature, it becomes steel.

f. It does not become black upon its surface, but equally

brown, by being wetted with liquid muriatic and other acids.

g. By solution in sulphuric and other acids, it affords a residue of less than $\frac{1}{300}$ of its weight of carbon; and if it could be obtained quite pure, there is no good reason to suppose there would be any residue at all.

II. *Steel* I understand to be that which has the following properties:

a. It is already, or may be rendered, so hard by immersion, when ignited, in a cold medium, as to be unmalleable in the cold; to be brittle, and to perfectly resist the file; also to cut glass, and afford sparks of fire on collision with flint.

b. In its hardened state, it may be rendered softer in various degrees (so as to be malleable and ductile in the cold), by ignition and cooling very gradually.

c. It requires upwards of 130° of fire of the scale of WEDGWOOD's pyrometer to melt it.

d. Whether it has been hardened or not, it is malleable when ignited to certain degrees: but when ignited to be white, perfectly pure steel is scarcely malleable.

e. It becomes black on its polished surface by being wetted with acids.

f. Much thinner and more elastic plates can be made of it by hammering than of iron.

g. The specific gravity of steel which has been melted and hammered, is in general greater than that of forged iron.

h. With the aid of sulphuric acid it decompounds a smaller quantity of water than an equal weight of forged iron.

i. It decompounds water, in the cold, more slowly than forged iron.

k. By repeated ignition in a rather open vessel, and by hammering, it becomes wrought or forged iron.

l. It affords a residue of at least $\frac{1}{300}$ its weight of carbon on dissolution in diluted sulphuric acid.

m. It is more sonorous than forged iron.

n. On quenching in cold water, when ignited, it retains about $\frac{2}{3}$ of the extension produced by ignition; whereas wrought iron so treated returns to nearly its former magnitude.

III. By the term *Crude*, or *Raw Iron*, I understand that kind of iron which possesses the following properties:

a. It is scarcely malleable at any temperature.

b. It is commonly so hard as to resist totally, or very considerably, the file.

c. It is not susceptible of being hardened or softened, or but in a slight degree, by ignition and cooling.

d. It is very brittle, even after it has been attempted to be softened by ignition and cooling gradually.

e. It is fusible, in a close vessel, at about 130° of WEDGWOOD's pyrometer.

f. With sulphuric acid it generally decompounds a smaller quantity of water than an equal weight of steel.

g. It decompounds water in the cold more slowly than wrought iron.

h. It unites to oxygen of oxygen gaz as slowly, or more slowly than even steel.

i. By solution in sulphuric and other acids, it leaves a residue not only of carbon, but of earth; which exceed the quantity of residue from an equal weight of steel.

k. It is perhaps more sonorous than steel.

With respect to interior structure:

I. *Wrought iron* is to be considered as a simple or undecompounded body, but it has not been hitherto manufactured quite free from carbon; which is to be reckoned an impurity.

The least impure iron, as indicated by properties, is that which possesses the greatest softness, toughness, and strength; but if it be soft, independent of combination, it will of course be of the toughest and strongest quality. To denominate it from properties, I would call it *soft malleable iron:* and from internal structure, it should be called *pure iron*, or *iron*.

The ore from the deep mines of Dannemora, produces the purest iron. It is in England called *Oeregrund iron.** It is almost the only iron manufactured which by cementation affords what our artists reckon good steel.

II. *Steel* has composition. It is a compound of iron and carbon, the proportions of which have not been accurately determined, but may be estimated to be one of carbon and 300 of iron. I would call this state of iron from external properties, *hard malleable iron:* and from interior structure and composition it may be called, as in the new system, *carburet of iron*.

Steel of the best imaginable kind is that which has not yet been manufactured: for it is that which has the most extensive range of degrees of hardness, or temper; the greatest strength, malleability, ductility, and elasticity; which has the greatest compactness or specific gravity, and which takes the finest polish; and lastly, which possesses these qualities equally in every part. Steel made by cementation, of the best qua-

* *Oeregrund* is not the name of the country in which the ore of this iron is gotten; or of the place where it is manufactured; but it is the name of a sea-port town, from which the iron of Dannemora was formerly exported.

lity, which has been melted, approximates the nearest to this kind of steel. Its greatest defect is want of malleability.

III. *Crude* or *raw iron* is a mixture, and has composition. It consists of pure iron united, and mixed with other substances so as to be hard unmalleable iron: but the substances with which it is almost always mixed and united are three, *viz.* oxygen, carbon, and earth. I would term this state of iron, on account of external properties, *hard unmalleable iron*; and on account of structure, *impure iron*.

In this statement of the interior structure of the different states of iron I have not thought it necessary to reckon the impalpable fluids, which they contain in perhaps different proportions; *viz.* light, caloric, electric, and magnetic fluids: for I believe their chemical agency has not been ascertained.

Iron may also contain a much greater quantity of carbon than has been above stated to be a constituent part of steel; and this state of iron is hard, unmalleable, and is not uniform in its texture. It may be called, according to the new nomenclature, *hyper-carburet of iron*. It is liable to be produced by cementing iron in a very high temperature for a very long time, with a large quantity of carbon; and it is also produced by melting iron, or steel, with carbon.

There are innumerable varieties of the first explained state of iron, viz. *wrought iron*. Some of these are familiarly known and distinguished by names among artists. Different quantities of carbon, which is here an impurity, are the occasion of these varieties; but as the carbon is not in sufficient quantity to diminish the toughness, softness, and malleability, to such a degree as to produce the obvious qualities of steel, such va-

rieties are reckoned to be those of wrought iron. The carbon may however be in such proportion as to produce a state of iron, which in some degree possesses the properties both of steel, and wrought iron; or which possesses partly the properties of steel, and partly the properties of wrought iron. It is quite arbitrary to call such kinds of iron, steel or wrought iron.

There are also innumerable varieties of the second state of iron explained, *viz. steel.* Some of these are known and distinguished by artists. A greater, or smaller, proportion of carbon, than the quantity requisite to saturate the iron, is the cause of these varieties: which are reckoned varieties of steel, because they possess in certain degrees the distinguishing properties of steel.

Besides these varieties of iron and steel depending upon carbon, there are other varieties from extraneous substances of a different nature. These are most frequently oxide of iron, or oxygen, and silica; especially in steel from the ore. The presence of phosphoric acid has been shown to be the occasion of the variety of iron, named *cold short*; which is brittle when cold, but not when ignited. And there is another variety called *red short*, which is malleable when cold, but brittle when ignited; the cause of which is supposed to be arsenic.

Iron and steel may contain an extraneous substance, and yet possess the properties of good, or even the best kinds of these metals: for this is the case when they contain manganese; as the fine experiments of Professor GADOLIN, made under the direction of BERGMAN, have demonstrated.

There are states of iron which are mechanical mixtures of steel and wrought iron. This is more or less always the case

with *bar steel*, made by cementation. If the bar be thick the interior part will be mere iron.

Lastly. There are different sorts of steel and wrought iron, from the difference of mechanical arrangement of their parts. So the specific gravity of steel by cementation may be increased by fusion, or hammering, and its grain altered. I have been told, that it may be hammered in the cold till it is so brittle that a slight stroke will break a thick bar. By quenching close-grained hammered steel in cold water, when ignited to whiteness, its specific gravity is diminished, its grain is opened, and it is rendered much harder.

These distinctions will perhaps serve to explain the nature of many varieties of the different states of iron, differently named by artizans, namely, *pig-iron; charcoal*, and *coal pig*, or *sow iron; blue, gray, white cast iron;—soft iron; tough iron; brittle iron; hard iron;—ore steel; cement steel; blister steel; soft steel; hard steel; hammered steel; cast steel; burnt steel; over cemented steel.*

I shall next endeavour to show to which of the above states of iron the wootz is to be referred, or to which of them it most approximates.

It appears that wootz is not at all malleable when cold; and when ignited it is difficultly forged and only in certain degrees of fire. It can be tempered and distempered, but not to a considerable extent of degrees (§ 3. *f, g*). The range of degrees of fire at which it is forged is of less extent (§ 3. and § 3. *c*) than the degrees at which it can be tempered, (§ 3. and § 3. *f, g*). It vies with the finest steel in its polish. Its specific gravity, which is less than that of ham-

mered iron, is very little diminished by ignition and cooling rapidly (§ 2. No. 6.). It melts, but at a higher temperature than crude iron (§ 3. *i*, *k*). It is not easily reduced into filings, even after annealing (§ 3. *g*). Its polished surface grows black by being wetted with acid (§ 5. *e*). It is not so brittle as raw iron, nor even as steel (§ 2.). On solution in sulphuric acid and water, it affords about the same quantity of carbon, and rather less hydrogen gaz than steel (§ 6.).

From these and other properties related in the preceding experiments and observations, it is evident that wootz approaches nearer to the state of steel than of raw iron; although it possesses some properties of this last substance.

With regard to the kind of steel to which wootz is to be referred; it is not of that sort in which there is either an excess or deficiency of carbon (p. 341, l. 15, *et seq.*); but it must contain something besides carbon and iron, otherwise it would be common steel. It appears that the solution in nitrous (§ 5.) and diluted sulphuric acids (§ 6.) contained only oxide of iron, and the residue of carbonaceous matter, as in common steel. Hence it is obvious to suspect that wootz contains oxygen, either equally united with every part of the mass, or united with a portion of iron to compose oxide; which is diffused throughout the mass. That this is really the ingredient in wootz which distinguishes it from steel, seems to be proved, or at least consists with its properties. For it accounts for the smaller quantity of hydrogen gaz than was afforded by common steel (§ 6.): it accounts for the partial fusion (§ 3. *b*): it accounts for the great hardness even on reducing its temper (§ 3. *g*); for its little malleability (§ 3.); perhaps it is the reason of the fine edge and polish

(§ 2, § 3.). The experiments (§ 3. *g*, *b*) confirm this conclusion. The oxide is not perhaps equally diffused, hence the wootz is not quite uniform in its texture and hardness, until it has been remelted (§ 3. *i*). The brittleness of wootz when white hot (§ 3. *g*) is a property of cast steel; and shows that it contains no veins or particles of wrought iron, and also that it has been melted. Common steel, which is all made by cementation, is very malleable, when white hot, only perhaps because it contains iron which has escaped combination with carbon.

The proportion of oxygen in wootz must be very small, otherwise it would not possess so much strength, and break with so much difficulty (§ 2.), and much more oxide would have melted out (§ 3. *b*). This oozing out of matter is analogous to that which appears on refining raw iron.

Although no account is given by Dr. Scott of the process for making wootz, we may without much risk conclude that it is made directly from the ore; and consequently that it has never been in the state of wrought iron. For the cake is evidently a mass which has been fused (§ 2.), and the grain (§ 2.) of the fracture is what I have never seen in cement steel before it is hammered or melted. This opinion consists with the composition of wootz, for it is obvious, that a small portion of oxide of iron might escape metallization, and be melted with the rest of the matter. The cakes appear to have been cut almost quite through while white hot (§ 2.), at the place where wootz is manufactured; and as it is not probable that it is then plunged in cold water, the great hardness of the pieces imported, above that of our steel, must

be imputed to its containing oxide, and consequently oxygen.

The particular uses to which wootz may be applied may be inferred from the preceding account of its properties and composition: they will also be discovered by an extensive trial of it in the innumerable arts which require iron.

XVIII. *Description of a Forty-feet Reflecting Telescope.* By William Herschel, *LL.D. F.R.S.*

Read June 11, 1795.

THE uncommon size of my forty-feet reflecting telescope will render a description of it not unacceptable to lovers of astronomy. I shall therefore endeavour to give as complete an idea of its construction as the limited compass of this paper will permit, and hope that, with the assistance of the annexed drawings, the mechanism of it will be sufficiently intelligible to such as have been in the habit of viewing machines and mechanical works.

It will be necessary to mention a few circumstances that led the way to the construction of this large instrument, in the execution of which two very material requisites were necessary: namely, the support of a very considerable expence, and a competent experience and practice in mechanical and optical operations.

When I resided at Bath I had long been acquainted with the theory of optics and mechanics, and wanted only that experience which is so necessary in the practical part of these sciences. This I acquired by degrees at that place, where in my leisure hours, by way of amusement, I made for myself several 2-feet, 5-feet, 7-feet, 10-feet, and 20-feet NEWTONIAN telescopes; besides others of the GREGORIAN form, of 8 inches, 12 inches, 18 inches, 2 feet, 3 feet, 5 feet, and 10 feet focal

length. My way of doing these instruments at that time, when the direct method of giving the figure of any of the conic sections to specula was still unknown to me, was, to have many mirrors of each sort cast, and to finish them all as well as I could; then to select by trial the best of them, which I preserved; the rest were put by to be repolished. In this manner I made not less than 200, 7-feet; 150, 10-feet; and about 80, 20-feet mirrors; not to mention those of the Gregorian form, or of the construction of Dr. Smith's reflecting microscope, of which I also made a great number.

My mechanical amusements went hand in hand with the optical ones. The number of stands I invented for these telescopes it would not be easy to assign. I contrived and delineated them of different forms, and executed the most promising of the designs. To these labours we owe my 7-feet Newtonian telescope-stand, which was brought to its present convenient construction about 17 years ago; a description and engraving of which I intend to take some future opportunity of presenting to the Royal Society. In the year 1781 I began also to construct a 30-feet aërial reflector; and after having invented and executed a stand for it, I cast the mirror, which was moulded up so as to come out 36 inches in diameter. The composition of my metal being a little too brittle, it cracked in the cooling. I cast it a second time, but here the furnace, which I had built in my house for the purpose, gave way, and the metal ran into the fire.

These accidents put a temporary stop to my design, and as the discovery of the Georgian planet soon after introduced me to the patronage of our most gracious King, the great work I had in view was for a while postponed.

In the year 1783 I finished a very good 20-feet reflector with a large aperture, and mounted it upon the plan of my present telescope. After two years observation with it, the great advantage of such apertures appeared so clearly to me, that I recurred to my former intention of increasing them still farther; and being now sufficiently provided with experience in the work I wished to undertake, the President of our Royal Society, who is always ready to promote useful undertakings, had the goodness to lay my design before the King. His Majesty was graciously pleased to approve of it, and with his usual liberality to support it with his royal bounty.

In consequence of this arrangement I began to construct the 40-feet telescope, which is the subject of this paper, about the latter end of the year 1785. The wood-work of the stand, and machines for giving the required motions to the instrument, were immediately put in hand, and forwarded with all convenient expedition. In the whole of the apparatus none but common workmen were employed, for I made drawings of every part of it, by which it was easy to execute the work, as I constantly inspected and directed every person's labour; though sometimes there were not less than 40 different workmen employed at the same time.

While the stand of the telescope was preparing I also began the construction of the great mirror, of which I inspected the casting, grinding, and polishing; and the work was in this manner carried on with no other interruption than what was occasioned by the removal of all the apparatus and materials from Clay-hall, where I then lived, to my present situation at Slough.

Here soon after my arrival, I began to lay the foundation,

upon which by degrees the whole structure was raised as it now stands; and the speculum being highly polished and put into the tube, I had the first view through it on Feb. 19, 1787. I do not however date the completing of the instrument till much later; for the first speculum, by a mismanagement of the person who cast it, came out thinner on the centre of the back than was intended, and on account of its weakness would not permit a good figure to be given to it. A second mirror was cast Jan. 26, 1788; but it cracked in cooling. Feb. 16, we recast it with particular attention to the shape of the back, and it proved to be of a proper degree of strength. Oct. 24, it was brought to a pretty good figure and polish, and I observed the planet Saturn with it. But not being satisfied, I continued to work upon it till Aug. 27, 1789, when it was tried upon the fixed stars, and I found it to give a pretty sharp image. Large stars were a little affected with scattered light, owing to many remaining scratches in the mirror.

Aug. the 28th, 1789. Having brought the telescope to the parallel of Saturn, I discovered a sixth satellite of that planet; and also saw the spots upon Saturn, better than I had ever seen them before, so that I may date the finishing of the 40-feet telescope from that time.

Description of the Instrument. See Tab. XXIV. to XLII. inclusively.

Fig. 1. represents a view of the telescope in a meridional situation, as it appears when seen from a convenient distance by a person placed towards the south-west of it.

The foundation in the ground consists of two concentric circular brick walls, the outermost of which is 42 feet in dia-

meter, and the inside one 21 feet; these measures are reckoned from the centre of one wall to the centre of the other. They are 2 feet 6 inches deep under ground; two feet 3 inches broad at the bottom, and 1 foot two inches at the top; and are capped with paving stones about 3 inches thick, and $12\frac{3}{4}$ broad. Fig. 2. represents a section of one of them.

These walls were brought to an horizontal plane by means of a beam turning upon a pivot fixed in the centre of the circle, which had a roller under it at the end. Upon this beam and over the roller was fixed a spirit level, to point out any defect in the walls; and by correcting every inequality that could be perceived, they were by degrees brought to be so uniformly horizontal that the beam would roll about every where upon them without occasioning any alteration in the bubble of the spirit level.

The timber of the groundwork (see fig. 3.), in the construction of which it was necessary to join strength to lightness, is put together in the following manner: three principal beams, A A, B B, C C, are extended from south to north, when the telescope is in a meridional situation. They are 43 feet 2 inches long, 6 inches broad, and 6 inches thick. The distances of the centre of the two outside ones is 17 feet. Within one foot of the ends of them are bolted down the cross beams D D, E E; which serve as a foundation to the two sets of ladders. These cross beams are 19 feet 2 inches long, 12 inches broad, and 6 inches thick; and by way of additional strength two more, F F, G G, of the same breadth and thickness, are bolted against the sides of the former, resting also upon the longitudinal beams, but these are rounded off at the ends, as marked in the figure.

The firmness of the foundation in the direction from south to north being thus secured, it became equally necessary to provide for strength in the support from east to west. For this purpose there are three latitudinal cross beams, H H, I I, K K, bolted down upon the former longitudinal ones. That which crosses the centre is 45 feet 2 inches long, and 12 inches broad; its thickness, like that of all the ground timber, being 6 inches. The other two are about 39 feet 9 inches long, and 6 inches broad. They project beyond the circular foundation wall about 8 inches, while the middle one projects 12. The use of these cross beams is to receive six supporters upon their respective ends, at the places which are marked with an ellipsis; the supporting beams which stand upon them being round and inclined towards the ladders, which they are to keep steady in the east and west direction.

Under each end of the principal beams at A A, B B, C C, H H, I I, K K, is placed a roller which rests upon the outer foundation wall. The three latter of these beams being placed higher than the former, have a piece of a proper thickness under the ends to bring the bottom of them to the level with the former. The rollers are set in iron frames, and bolted to the beams, so as to be directed to the centre of their motion. They are 8 inches long, and 6 in diameter. The construction of those which are under beams that come from the centre, is expressed in fig. 4.; but the irons which hold the rollers under beams in other directions are more or less eccentric, as may be seen in fig. 5.

No other fastening in the whole machinery of the woodwork has been admitted but screw bolts; as tenons of any kind, in an apparatus continually to be exposed to the open

air, will bring on a premature decay, by lodging wet. In order to obtain steadiness, however, the cross timbers of the frame, in all places where they are bolted together, are let in, and receive each other about $\frac{3}{4}$ of an inch, which makes an entering of $1\frac{1}{2}$ inch into each other, and produces the required firmness without any material weakening of the timber.

The twelve rollers whose place has been pointed out, would not have been sufficient to support the length of the beams to which they are fastened, the shortest of which, as we have seen, being near 40 feet long. Eight additional rollers, therefore, sustain the ground timber half way towards the centre at L M N O P Q R S. They are, like the former, directed to the pivot upon which this frame moves, and rest upon the inner foundation wall.

In the centre is a large post of oak, framed together with braces under ground, and walled fast with brick-work so as to make it steady. The two central beams B B, I I, cross each other over this post; and a strong iron pin, or pivot, goes through them both into a socket within the centre of the post, so as to permit the whole of the foundation timber to turn freely upon this centre, when a proper force is applied for that purpose.

Although by means of the 20 rollers, and this support in the centre, the bottom frame of the stand to the telescope be firmly supported, it may notwithstanding be easily seen that there was occasion for some additional braces, in order to keep each beam in its proper situation. For this reason 8 pieces, *a a, b b, c c, d d, e e, f f, g g, h h,* of a proper length, 4 inches broad and 6 deep, are applied near the end of the beams against the sides of them; these are held together by irons

that are bent, as in fig. 6. One of them, for instance, is bolted down, at *a* and *b*, fig. 3 upon the braces that hold the beam H in its place; upon which it is also screwed down, and makes a firm joint of the three pieces. A small entrance of *a* and *b* into H takes off the weight from the iron, and keeps the braces in their places, when bolted together.

Two other braces, *i i*, *k k*, are added. Their use is evident in the horizontal motion of the telescope; for that being effected, as will be seen hereafter by means of the strong centre beam I I, the connection of the whole frame with this beam is completed by the pieces *i i*, AA, BB, CC, *k k*.

Before I proceed to explain any other part of the work in this figure, it will be necessary to describe the construction of the ladders and their braces.

Fig. 7. represents the front set of ladders. $\alpha\,\beta\,\gamma\,\delta\,\varepsilon\,\zeta$ are six tapering halves of three large poles, or rather small masts, cut through the middle. Before the masts were cut they measured between 11 and 12 inches at the bottom; but when they had been sawed through, the pieces were flattened on the front and back, so as to be reduced to 8 inches at the bottom, and $5\frac{1}{2}$ at the top, while the other dimension, or thickness, was left to its full extent. By trimming up and making them pretty equal, however, they became also reduced to about $5\frac{1}{2}$ and 5 inches in that direction.

The length of the ladders is 49 feet 2 inches, and their construction is as follows. The top of each step is 9 inches from that of the one below it; and, beginning 12 inches from the bottom, there are two rounds and one flat placed alternately, as far as 40 rounds, and 19 flats. In the place of the 20th flat is the centre of the meeting of the front and back sets of

the ladders; above this is another flat, with a termination of 16 inches at the top.

The timber of the sides being tapering, a similar diminution of the flats and rounds has been attended to, especially as their size, in proportion to the sides, is far above what is generally used in building ladders. The flats and rounds are all made of solid English split oak.

The lowest rounds are 2 inches thick in the middle, and $1\frac{1}{2}$ where they enter the sides. At the 21st round the thickness is $1\frac{3}{4}$ in the middle, and $1\frac{3}{8}$ at the shoulder. About the 31st round the thickness is $1\frac{1}{2}$ in the middle, and $1\frac{1}{8}$ at the shoulder, and this size is nearly preserved up to the end. Those parts of the rounds which enter the sides of the ladders have all been turned in a lathe, and are about $\frac{1}{8}$ of an inch tapering, in order to fill the holes properly, which were also made a little tapering so as perfectly to answer the size of the rounds.

The lowest flat, for a particular purpose in the erection of the ladders, which will be explained hereafter, is $4\frac{1}{2}$ inches by 2. The next, as far as the 10th, are $3\frac{1}{8}$ by $1\frac{5}{8}$; from the 11th to the 16th they are $2\frac{3}{8}$ by $1\frac{1}{4}$; and from the 17th to the last $2\frac{1}{2}$ by $1\frac{1}{8}$ inches.

The two outside divisions of the ladders serve for mounting into the gallery, and therefore contain rounds as well as flats. The distance of the sides, the flat parts of which, as in common ladders, are put facing each other, is 18 inches, and remains the same up to the end. But the two inside divisions, which have no rounds, are placed with the flat face outwards, and the distance between these faces being 2 feet 8 inches up to the top, the parallelism of these divisions is preserved outside, while that of the mounting ladders is continued within.

The reason of this arrangement is, that the brackets which support the moveable gallery rest upon the inside frames of the ladders. These go upon 24 rollers, as will be seen hereafter, and 12 of them confining the gallery sideways, while the other 12 support it, the parallelism was of course required where it is placed. The mounting ladders are made parallel within, that a moveable chair, intended to be made if required, might be drawn up with a person seated in it, to prevent the fatigue of mounting, or take up in safety any one who chanced to be afraid of ascending an open ladder.

The back set is constructed like the front; and, the ladders being of the same length, the only difference is that no rounds have been put into them. The flats have been preserved on a double account; first, that the connection of all the side timber might be firm and strong; and secondly, that every part of the frame might be accessible. For by means of these flats we have steps of 27 inches, which may be ascended with tolerable ease, when occasion requires.

The method of joining the front and back at the top, is by passing one set of ladders through the other so as to embrace it; the backs, therefore, which go outside, are placed a little farther asunder than the fronts; and the same pins pass through them both, at $\eta\,\kappa$ and $\theta\,\iota$, where a section of the back ladders is shewn, with the pins going through them. The last flat was put into the ladders after they had been erected and secured together.

The method of setting up and bracing the ladders was as follows:

When the eight principal beams of the groundwork A B C D E H I K, fig. 3. had been put together, the eastern front

ladder $\delta\,\epsilon\,\zeta$, fig. 7. with its back $\xi\,o\,\pi$ (not expressed in the figure) bolted to it, was laid down edgeways, with the ends δ and ξ opposite to the same letters in fig. 3. and the centre θ towards the west. In this situation the ends were properly secured to the foundation beams, that they might not slip. The centre θ was then raised up to about 10 or 12 feet from the ground, and a tackle was fastened on one side to the crossing of the ladders at ι, and on the other, about 2 feet from the ground, to a tree at a convenient distance in the east. By assisting a little at first, in lifting the centre, our tackle soon got hold of the ladders, and drew them up. A rope had been provided to prevent their going farther than the perpendicular; and being secured in that position, the tackle was now fastened to the other ladder at η; but instead of making use again of our tree, the corresponding tackle was secured on the top of the first ladder at θ; by which means we easily drew up the second. Both sets of ladders stood now upon the ground, within the frame, and with the front legs, $\alpha\,\beta\,\gamma\,\delta\,\epsilon\,\zeta$, nearly opposite to the same letters on the front beam; while the legs of the back stood opposite the letters $\lambda\,\mu\,\nu\,\xi\,o\,\pi$ on the back beam.

We now proceeded to put on the middle top cross-beam, which is placed above the two sets of ladders in the angle made by their crossing each other. It is expressed by points in fig. 7. and may be seen in its place, fig. 1. The method of keeping it there, and securing the proper distance of the ladders by this beam, which is of a cylindrical form, is as follows: twelve iron loops, shaped to the ends of the ladders, with arms to them like lamp-irons, and a hole at the end of each arm, are slipped down upon the ends of the ladders, till two and two of

them, as *a b*, fig. 8. meet in the middle of the cross-beam *c*, which is about 8 inches in diameter. Here a screw bolt, coming up through the beam, passes into the holes of the two irons, where all is screwed firmly together. By this means no holes are made to weaken the tapering ends of the ladders, and the centre beam takes firmly hold of every one of them; so that were even the pins η and θ pulled out, the ladders would still remain firmly kept together.

Before, however, the ladders were screwed to the centre cross-beam they were lifted up into their places upon the front and back foundation beams D D, E E. This was done by a strong lever-beam, about 25 feet long (see fig. 9.), with two moveable iron claws, *a b*, at the end; which took hold in two places, equally distant from the middle division of the lowest flat of the ladders: this flat having been made, as has been noticed before, sufficiently strong for sustaining the whole weight of a set of ladders. Thus they were lifted one by one into their proper places, and supported till they could be shaped with their lower ends to fit upon their respective bearings, and were in the same manner brought to the required parallel situation: this kind of lever affording the means of giving some small motion to the weight it sustains, not only upon the pivot *c*, but also on the support *d e*, which is rounded off at the bottom.

When the ladders had been properly adjusted to their places, we proceeded to support them immediately by two capital side braces. These consist of two whole masts, of nearly the same dimensions with those which were sawed through for making the ladders: the upper end of each was mounted with an iron loop *a*, two claws *b c*, and ring *d*, which were put on with

bolts, *e f*, as expressed in fig. 10. The poles being drawn up, the loops were put upon the centre pins *ι κ*, fig. 7. and keyed on; while the lower ends of the poles were lifted into their places, on the cross foundation beam I I, fig. 3. and fitted upon the elliptical marks at the ends of them at 1, 2.

Before the ladders and side braces were fixed down, a line with an hundred weight at the end, immersed in a tub of water, was hung upon the centre at *θ*; and being viewed from a considerable distance, was made to range with the flat side *δ θ* of the ladder. The bottom of every end of the bracing poles and ladders being finally adjusted by this plumb-line, they were all screwed down by strap-bolts, as delineated in fig. 11. The top centre cross-beam was now also screwed to the loops on the ends of the ladders, which, as we have mentioned before, see fig. 8. had been already prepared.

The most essential part of the stand being now erected, we proceeded to brace and support it finally. Four small ladders, but without rounds, 22 feet 9 inches long, having been made, were erected to support the large ones half way. They consist of three half-poles each, placed at the same distance from one another as the large ones, and have their faces turned like them. These meet the former at the 10th flat with their upper ends; while the lower parts of them rest upon the middle foundation beam in fig. 3. at 7, 8, 9, 10, 11, 12; 13, 14, 15, 16, 17, 18. They are screwed both at the top and bottom with flat corner-irons as in fig. 12. and their situation may be seen in fig. 1.

In the next place four less poles were now added to support the ladders sideways. They stand upon the beams H H, K K, at 3, 4, 5, 6; and are fastened against the ladders at the 10th

flat, or half way up. The upper end is secured with an iron, as in fig. 10. through the loop of which passes a strap-bolt that holds it at the same time with the triangular brace, which will be described, to the ladders. The lower end is strap-bolted down upon the beams, as in fig. 11.

The two long side-bracing poles are supported each by two less poles, which meet them at one half and one quarter of their length from the bottom; or at a height opposite to the 5th and 10th flats of the ladders. Those poles which meet the great ones in the middle are placed with their lower ends upon the middle beam at 19 and 20; and the shortest rest at 21 and 22. They add very materially to the steadiness of the frame in the east and west, or lateral direction; at the bottom they are also fastened by strap-bolts as in fig. 11. and at the top by loops, as in fig. 10. They may also be seen in fig. 1.

The next braces we are to describe are those of the sides of the ladders, and these it will be seen by fig. 13. are of so simple a nature, that a bare inspection of their representation will be sufficient. The size of the horizontal pieces is 6 inches by $3\frac{1}{2}$; but those which are parallel to the ladders, and are of no other use than to keep the rest in their places, or as it were brace-bracers, are only $3\frac{1}{2}$ by $2\frac{1}{2}$.

Besides these there are three sets of braces, which serve to confine the poles to their stations. The highest set meets the side brace of fig. 13. at the 15th flat. The next meets the middle brace at the 10th flat, and both these make with the here mentioned side braces a triangle, in the vertex of which is inclosed the large pole that is braced by them. At the 5th flat a third set of braces, which incloses the two small

poles as well as the large one, is carried round with four divisions. In order not to weaken the great pole by many holes, the braces secure it by a double iron strap, *a b c d*, fig. 14; and the small supporting poles which rest upon 19, 20, 21, and 22, fig. 2. are at the same time joined by a single screw-bolt, *e f*, which passes through the loop *g h i* at the end of them, and through the straps which hold the braces to the great pole; see fig. 1.

The back of the ladders is bound together by a large cross, *a b*, *c d*, from the 10th flat to the middle braces, and by two horizontal pieces, *e f*, *g h*, as represented upon a small scale in fig. 15. The cross is bolted in twelve places to the ladders, and the horizontal pieces in six places each. The size of these braces is 6 inches by 4; but the lowest horizontal beam, which is used for a point of suspension to lift the mirror in and out of the tube, is 6 inches and a half by $5\frac{1}{2}$; and the bolts that hold it to the ladders are also very substantial.

The front of the ladders, it is very evident, would admit of no brace, and is left entirely open for the tube of the telescope to range in. It receives, however, some confinement from the moveable gallery, which is always hung across the front, in the place where observations are to be made.

This gallery is next to be described. It consists of three separate parts: two double side brackets with a small platform upon them, and a middle passage. The whole of it when joined together is properly railed in at the front by wooden palisades; and on the inside by light iron-capped bars. Each of the brackets by which the gallery is supported consists of three frames; a parallelogram for the bottom, with two trian-

gular sides erected on the former, and held together by a narrow platform on the top. Fig. 16. represents the bottom frame. Its length, *a g*, is 8 feet 10 inches, and breadth, *g b*, 2 feet 8 inches. It is made of yellow fir deal, 4 inches broad, and 2 inches thick. Six sets of brass rollers, in iron frames, constructed as represented in fig. 17. by means of the two small screw-bolts, *i k*, are screwed under the frame, fig. 16. at *c d e f g b*; so that when this frame comes to be placed upon the front of the ladders at $\beta\gamma$, or $\delta\epsilon$, fig. 3. the 6 rollers in the direction *m*, fig. 17. will sustain the frame, while the other six, in the direction *l*, by embracing the flat sides of the ladders, which as has been described are turned outwards for this purpose, will prevent the frame from slipping off sideways, when it rolls up and down the ladders.

Two such triangles as delineated in fig. 18. the wood of which is $3\frac{1}{2}$ by $2\frac{1}{2}$ inches, are fastened to the frame, fig. 16; one, with the side *n p* upon *c g*, the other upon *d b*. These being joined at the top by the platform of boards, screwed down to the supporters *q r s*, of that which is represented here, and of the corresponding one, which rests on *d b*, complete the bracket.

Upon the platform are fixed palisades commencing at *t*, 19 inches from *q*; which turn the corner at the front *s*, and are continued so as to meet the middle platform of the gallery. The palisades over *t r s* are strengthened and rendered steady, by a seat which is fastened against them, and supported from the floor by slight iron bars.

The other double bracket, with its platform, palisades, and seat, which runs upon the right side of the front ladders, is in

every respect the same as that on the left, except that the entrance is here upon the right side, instead of being at the left in the former.

The whole gallery together, the floor of which is represented in fig. 19. takes up a space of 13 feet 6 inches broad, by 6 feet 1½ inches in depth; the middle platform, however, is cut away so as to leave sufficient room for the tube to come forward in high altitudes. At *a c* and *b d* it is 4 feet 3 inches, but at *e f* and *g h*, a space of 4 feet 10 inches long, it is only 2 feet deep. The front, *c f h d*, contains palisades, which meet those of the left bracket *t r s c* at *c*, and the similar ones of the right at *d i k*. These palisades are 3 feet 2 inches high. The light iron rails on the inside pass along the edge, *l a e g b m*, and are only 2 feet 3½ inches in height.

The first requisite in this gallery being that it should be drawn up to any required altitude, it became necessary to connect the two double brackets and the middle platform in such a manner as to bear some little derangement in their level, arising from the inequality of the motion of the side brackets. With a view to this end, the method of uniting the parts is as follows. The dotted lines 1, 2, &c. shew the place of the joists which support the floor of the platform. At the ends, 1, 2, 5, 6, 7, 8, of these joists are six iron hooks, shaped as in fig. 20; they are bolted and screwed with the end *n o* under the bottom of the joists, and rise to the level of them with the arms *p*, leaving the hooks *q* projecting. These enter into six proper openings made in the side brackets; three in each: they leave a space of about ¾ of an inch between the two brackets and the middle platform, which permits a small irregularity in the level of the three parts to take place without injury to either of them.

The hooks sink down into the floor of the sides so as to be level with the surface; and go over the inside of the supporting triangles, fig. 18.; which, for the sake of additional strength, and to prevent their being galled by friction, are lined with an iron plate at the inside, in all their length.

The light iron rail joining the bars of the inside, which are along the margin *l a e g b a*, fig. 19. are left moveable at the bottom, in the places *l a* and *m b*; where they run down into loops; by which means they admit of being a little displaced.

The contrivance to make the junction of the front and side palisades moveable is by means of a front bar. This being slipped upon pins at the end of the rails belonging to the sides, a hole at each end of the bar, lined with an iron plate about 2 inches long, through which the pins pass, permits the bar to be drawn either way. There are moreover at the ends of the rails, which are fixed to the platform, two iron hooks; which, though they bind the rails to the front bar, still permit it to go up or down a little way, as occasion may require. By this means a deviation from the level, amounting to six or eight inches, will occasion no injury to the wood-work. The greatest security against such a derangement of the platform, however, will be explained hereafter, when we come to the mechanism by which it is moved.

There is a small staircase by which we may ascend into the gallery, without being obliged to go up any ladder; and as that is strong enough to hold a company of several persons, and can afterwards be drawn up to any altitude, observations may be made with great conveniency: the activity of an astronomer, however, will seldom require this indulgence. The readiness with which I ascend the ladders, has even prevented

my executing the projected running chair, which may easily be added, to take a single person into the gallery after it has been already drawn up to its destined situation. A view of the staircase in fig. 1. will suffice to point out its construction. I ought only to observe, that in the engraving the gallery is placed higher than where it will join the staircase properly, but that when it is lowered on purpose, it becomes then to be just one step above the little landing-place of the staircase, and the palisades of the former unite with the railing of the latter.

The next piece to be described, is the tube of the telescope. This, though very simple in its form, which is cylindrical, was attended with great difficulties in its construction. No one will wonder at this who considers the size of the tube, and the materials of which it is made.

Its length is 39 feet 4 inches; it measures 4 feet 10 inches in diameter, and every part of it is of iron. Upon a moderate computation, the weight of a wooden tube must have exceeded an iron one at least 3000 pounds; and its durability would have been far inferior to this of iron.

The body of the tube is made of rolled, or sheet iron, which has been joined together without rivets; by a kind of seaming, well known to those who make iron funnels for stoves. It is represented by fig. 21. where the two sheets of iron are left a little open at *a b* to shew the construction, but which being properly compressed will become very nearly flat: the whole outside was thus put together in all its length and breadth, so as to make one sheet of near 40 feet long, and 15 feet 4 inches broad. The tools, forms, and machines, we were obliged to make for the construction of the tube were very numerous. For instance, in the formation of this large sheet, a kind of

table was built for its support, which grew in size as the sheet advanced, till when finished, it was as large as the whole of it. In the formation of the sheet, cramping irons, seaming bars, setting tools, and claw-screws, such as are represented in the figures 22, 23, 24, 25, and 26, were made in great number, to confine and stretch the parts as they were seamed together. The small single sheets of which this large one is composed, are 3 feet 10 inches long, and about 23½ inches broad. Their thickness is less than the 36th part of an inch; or, what will be a more precise measure, a square foot of it weighs about fourteen ounces. They are joined so, that the middle of a whole one always butts against the seam of the preceding two, in the manner of brick-work, where joints are crossed by bricks above and below.

When the whole sheet was formed, which was done in a convenient barn not far from my house, the sides were cut perfectly parallel, and afterwards bent over at the ends in contrary directions, as in fig. 21. to be ready to receive each other. A number of broad hooks, such as were proper for grasping the sides of the sheet, with loops at the other end, for cords to go through, see fig. 22. were now prepared with their necessary tackle.

Twelve pulleys were fastened about 11 feet high, on moveable beams, that might be drawn together; six on each side. The sheet was now taken up, by occasioning all the corded hooks to be drawn at the same time, and while it was kept suspended our large table was taken to pieces. Another kind of support was now put under the middle of the sheet to receive it. The form of this was that of an hollow segment, or quarter of a cylinder, cut lengthways, to the extent of a few

feet more than the length of the intended tube; and the concavity of which was formed by the same radius as that of the tube.

The sheet being let down, it rested upon the hollow gutter; for so we may call the machine that was placed under it. Six moveable segments of a whole cylinder, or circular arches, about 3 feet wide each, which had been prepared, were now brought upon the sheet and placed at proper distances from each other. By these the sheet was pressed down upon the foundation, so that no injury could be done by walking upon it. The beams which held the pulleys were now brought close together; which being done, we hung the pulleys of one upon the hooks of the other beam, so as by that means to cross the cords which held the sheet. In this operation we slackened only one of the cords at a time, the rest being sufficient to keep the whole up.

The beams were now again separated, and the cramping hooks by the crossing of the cords drew the two sides of the sheet together.

Here I must take notice, that the circular inside supports, which resembled the machines upon which arches of brickwork are built, were cut in two in the middle, as in fig. 27.; some part of the circumference being taken out, that when they were laid down upon each other they might not fill the tube. Four long wedges, *a b c d*, in opposite directions, were confined two and two in the notches *e f*, *g h*; and similar ones at the back. By driving them in very equally, the upper half of the arches might be forced up so as to swell to the full extent of the tube.

When all this was properly arranged, and the arches lowered,

the two sides of the sheet were gradually brought to take hold of each other. As we proceeded, the wedges within the arches were forced in successively, till at last, with much care and considerable difficulty, the two sides completely embraced one another, and were kept stretched by the swelled inside arches.

Another circular arch, closed in with boards all around, well rounded off, and only about 2 feet 3 inches long, had a vacancy at the top into which we could introduce the iron seaming bars, fig. 23. for indenting, and 24. for closing up the long seam of the two sides. This arch also had its stretchers for swelling it up, and served at the same time, as soon as the seam was properly closed, to beat with mallets the whole sheet all around upon its well-finished outside, in order to take away any accidental bulge which it might have received in the long preparations it had undergone, till it came to the present state.

The same arch, as soon as any portion of the tube had been done, was removed to another place, and the whole was by this means completely seamed up.

The theory upon which the strength of so thin a cylinder of iron is founded, is, that the sides of it must unavoidably support it, provided you can secure the cylindrical form of the tube.

It appeared to me the most practical way to obtain this end by the following contrivance. By a few experiments I found that a slip of sheet iron, a little thicker than that of the tube, and doubled to an angle of about 40 degrees, as in fig. 28. might afterwards be made circular, as in fig. 29. The deepest we could conveniently bend, and such as I supposed would answer the end, was when the sides *a b* were about $2\frac{1}{2}$ inches

broad. They were shaped red hot upon a concave tool, which had the required curvature and angle of the slips. The pieces were long enough to form a complete quadrant of the circle, with the ends sufficiently projecting to be seamed together.

Before they were joined the sides received another bending, as in fig. 30. which was given them by tools of a proper convexity. A back was next prepared, consisting of a slip of iron turned up at both sides, and also bent to the circle, as in fig. 31. Last of all, the four quadrants having been put together, and a back put round them, the whole was firmly seamed together, so as to resemble a hollow triangular bar made into a hoop or ring, of a proper diameter to go closely into the tube, so as to keep it extended, and braced to the cylindrical form. A section of the ring with the bottom seam not quite pressed down, in order to shew it better, is represented in fig. 32.

One of these rings was put into the middle of every one of the small sheets, which brought them to about 23 inches from each other. They were carried in edgeways, and afterwards turned about and forced into their respective places. In order to get them in, as they were all obliged to go in from one side, there was substituted, in the room of the circular arches, a kind of temporary props, like fig. 33. that could be easily removed, one at a time, and were narrow enough at *a b* to pass through the hoops while they advanced ; and as soon as a ring was in its proper place, no further support became necessary.

In this manner we secured the cylindrical form of the tube ; and as soon as this was accomplished, we had every thing removed from within and without, and began to give the tube three or four good coats of paint, inside as well as outside ; in

order to secure it against the damp air, to which it was soon to be exposed.

As the tube was now much lighter than it would be hereafter, we transported it into my garden in the following manner. Many short poles, about 5 feet long each, were joined two and two by a piece of coarse cloth, such as is used for sacks, about 7 feet long each. This, being fastened in the middle, left at each end part of the pole to serve as a handle for a person to hold by. The cloth of one of these being put under the tube, there was left one of the poles at each side, and four men taking hold of the ends of the poles, might conveniently assist in carrying the tube. When six sets of these were put under the tube, it was with great facility lifted up by 24 men, who carried it through an opening which we had made at one end of the barn. The inclosure of part of my garden having also been taken down, with some trees that were in our way, it was safely landed upon my grass-plot; where a proper apparatus of circular blocks was put under to receive it. While it remained in this state, we prepared every thing for its reception, and afterwards moved it into its place, and supported it in an horizontal situation.

It will be necessary now to return to the rest of the machinery, which by this time was in great forwardness.

Two solid cast-iron concave rollers, $6\frac{1}{4}$ inches broad, and 10 inches in diameter, are mounted upon an axle or iron bar, $2\frac{1}{2}$ inches square; the axle in the middle being swelled out so as to admit of a pivot $2\frac{1}{4}$ inches thick to pass through it, without being weakened by the hole. The tube is mounted upon this at the lower end, and as the speculum lies in this

part, great strength is requisite to support it firmly, as also an extensive connection of this strong part with the length of the tube. The speculum likewise is to be put in here, and when the telescope is in use the cover of the speculum is to be taken off, and afterwards to be put on again; for which reason a convenient door or opening must be had. The line of collimation of the mirror also requires an adjustment at this end of the tube; and a small side motion is required upon the pivot of the axle, which must not only be perfectly smooth, but equally firm and steady. All these exigencies have been provided for in the following manner.

Fig. 34. represents the back of the tube, closed up by six iron bars, *a b c d e f*, which cross each other. The middle bar is 4 inches broad, and $1\frac{1}{8}$ thick at *e*, but is swelled so as to measure 5 broad, and $1\frac{1}{2}$ thick at *l*, where it is turned at rectangles, and passes under the bottom of the tube. In this bar is a square hole, through which a pivot, or pin, passes from the inside of the tube, where it is confined by a square head, into the hole of the axle, A B, under which at the bottom it is keyed fast at C; with proper washers between the joints to allow of a very smooth motion.

The bar, *c g*, is of the same strength with *e l*, and passes over it at *e*. It is bent at rectangles at *c* and *g*, so as to pass along the sides of the tube. The two bars, *d m*, *f k*, fastened upon *c g*, and afterwards turned down to the back, are 2 inches broad, and $\frac{5}{8}$ thick; and are also bent at rectangles at *m* and *k*, so as to go under the tube: the remaining two bars, *b b*, *a i*, cross the other three bars, with proper offsets; and are bent at rectangles on both sides, that they may turn round the end of the tube, to go along the sides of it. At the crossing

of these bars they are fastened by screws, which pass through the upper bar, and are lodged in the lower one. The same screws pass on through a moderate plate of sheet iron which closes the back, and is held by nuts upon them within the tube. The eight returning bars, at *a b c g h i k*, extend only to about 6 inches along the tube; but they are immediately received by other eight bars of the same size, which are screwed upon them. These bars are made tapering, so as only to measure $1\frac{1}{2}$ inch broad, and $\frac{3}{8}$ thick at the ends; and they are 9 feet 8 inches long. The middle bar is turned over about 16 inches, and made tapering; and the bar which meets it is laid under it, and also made tapering to answer the former. The pivot goes through both, and they form, as it were, only one bar; this is soon reduced gradually, and at the end measures $1\frac{3}{4}$ inches by $\frac{1}{2}$; its length being the same as the rest.

The segment *c n g* is cut off to leave an opening, which is 2 feet broad at the sides. A cover of the same shape, with the piece cut out of the tube, is laid upon the place, to overlap the opening properly. But this would not have been sufficient: for, after observations at night, this cover, though close enough to preserve the inside of the tube from damp or wet, would itself be covered with dew or condensed vapour. And by taking it off in order to secure the mirror, many drops of water would unavoidably fall upon it from this wet cover. To prevent this, an outward cover has been applied, which completely preserves the inner one from moisture.

The tube being much too weak, in this place, for the support of the mirror, a piece consisting of three sheets of iron, 2 feet 4 inches broad, $\frac{1}{8}$ thick, and dove-tailed together so as to be long enough to reach from *c* around *b a m l k e b* to *g*,

was added to its thickness within; upon this again, an iron bar, 6 inches broad, and $\frac{5}{8}$ thick, was bent round close to the end, from *b* to *b* only; and another bar, $2\frac{1}{8}$ inches broad, and $\frac{1}{2}$ thick, made into a complete circle, was added to support that end of the tube which had been cut, to make the entrance. All these pieces were well secured, by screw-bolts passing through the nine long outside bars *a b c g b i k l m*, next through the tube, then through the strong sheet, and at last through the broad strap and circular bars, upon which they were screwed down with nuts at the inside. The more advanced parts of the long bars were secured also by screw-bolts passing through the tube, and through circular straps of hoop iron, about $2\frac{3}{4}$ inches broad, and $\frac{1}{8}$ thick; one of these being put into every sheet of the tube as far as the bars went.

As we had now secured what I call *the point of support*, it was no less necessary to form a strong *point of suspension*. This was obtained by grasping the tube with a system of bars similar to that which has been employed at the bottom.

Ten bars, equally divided around the circumference, about 10 feet 4 inches long, are placed longitudinally so as to have one of them at the top, and an opposite one at the bottom. Every one of these has six screw-bolts, which pass through the bar and the tube, and also through complete circles of hoop iron, which is of the same breadth and thickness as has been mentioned before. The bars also, except the highest and lowest, are of the size of those which have been used about the point of support. They are also, like them, chamfered at the sides, and begin to lessen in breadth and thickness about 4 feet from the front, to the same dimensions with the former.

The lowest bar is a little stouter in its dimensions, but otherwise exactly the same.

The middle bar at the top is strongest about the point of suspension, where it is 4 inches broad, and 1 inch thick. In this place it is crossed by another bar, which is a segment of a circle, and embraces the middle one, and two other bars at each side. This crossing bar is $3\frac{1}{2}$ inches broad, and 1 inch thick in the centre; chamfered or sloped at the sides, and reduced in thickness towards the ends. It passes over the middle bar with a proper offset, and its two ends terminate upon the two farthest bars; but the bars next to the middle, on each side, are made to pass over it. The middle bar receives a loop, by which the telescope is suspended, the centre of which is 3 feet 8 inches from the mouth of the tube. The loop is made of iron, 4 inches broad, and 1 inch thick; doubled together, and the ends of it opened again, so as to cross the circular bar, and to rest upon the strong middle one, to which it is fastened with four large screw-bolts. These pass through the bar into the tube, where they are well secured with substantial nuts. The long middle bar is reduced gradually after the place of the loop, the ends of which extend about 18 inches, till it comes at last to the breadth of $2\frac{1}{4}$ inches, and thickness $\frac{3}{8}$.

All the ten bars are secured with six screw-bolts each, which pass through the tube, and through iron hoops, four of which are of the same dimensions with those which are used about the point of support. The hoop which is under the suspension is 8 inches broad, and a little thicker than the rest. The front hoop is of a different construction: its thickness is

about two-tenths of an inch, and being bent at rectangles, that part which is held down to the tube, and to the ten bars, keeps it steady, while that in the other direction serves as a ring, both to strengthen and confine the aperture. It projects about three inches all around, and leaves an opening of 4 feet 4 inches to the mouth of the telescope.

The loop of suspension stands across the tube, and receives a round bar of iron, shaped as in fig. 35. which is left at liberty to take its own position. To the places *a b* are hung two double pulleys, and at *c*, a single one; all turning upwards to meet the upper set of pulleys.

On the top of the stand, and round the centre beam, passes a ring of iron, 4 inches broad, and 1 inch thick, which contains a loop resembling that on the point of suspension at the telescope. This also receives a round iron bar, bent as in fig. 36. and supports three double pulleys at *d e f*.

Nothing can obstruct the motion of a tackle more than the friction of the ropes against each other; and as the utmost ease was required in the action of my pulleys, it was particularly necessary to guard against a defect of that kind. Another inconvenience was to be avoided, still more pernicious than the friction of the tackle. When pulleys are set, two, three, or four in a row, side by side, they will incline one way when the weight is drawn up, and another when it is let down. This may easily occasion an accident, which in the case of my large telescope must have been exceedingly troublesome, and probably in the end proved fatal; for by the side inclination of the set, a rope will sometimes slip out of its place; especially as my ropes are well soaked in melted tallow to preserve them from moisture. This in summer will occasion dust to

settle upon them, and sometimes fill up a channel of the pulleys, so that the least deviation from the perpendicular may throw a rope out of its place. Should this happen in the night, when it might not be immediately perceived, the rope would soon be injured, or even cut through, by the continuation of the force that acts upon it. Besides, this irregular motion of the pulleys, when the telescope is finely suspended in the meridian, will tend to produce a little deviation in right ascension, which ought to be avoided. My pulleys, therefore, are all but one in a meridional situation, and this might also be turned the same way if there were occasion for it. The double pulleys are placed under each other; by which means the stress of the lower ones at the top, and the upper ones at the bottom, adds to their meridional and perpendicular steadiness.

In order to command every altitude, from the horizon to the zenith, it was necessary that the point of support should be moveable. Its motion is effected by a mechanism which I shall now explain.

Eight bars, $2\frac{1}{2}$ inches broad, and $1\frac{1}{8}$ thick, were cut into teeth at the distance of $1\frac{1}{2}$ inch each; and afterwards connected by slips screwed against both sides of the places where two butt together. Their length is such, that four and four being joined make up two bars of 29 feet 8 inches long each. Two loops which are screw-bolted to the ends of them, take hold of the axle, fig. 34. at D and E, which in those places is made round for that purpose.

Upon the foundation beams in fig. 3. are fixed four short cross beams, at *ll mm nn oo;* these carry the following machine. A handle which turns a pinion of eight leaves, drives a wheel of 20 inches diameter, with 51 teeth; the axle of the wheel

contains a pinion of 12 leaves, driving a wheel 3 feet in diameter, of 88 teeth. On the axle of this latter are fixed, upon a long bar, two lantern pinions of twelve leaves, at a distance of 3 feet 9 inches from each other, and these are confined down to work in the two long cut bars, which pass under them at that distance in iron notches, to prevent their receding sideways. The long bars are supported by narrow slips of timber, *p p*, *q q*, which are extended from the front to the back; as otherwise the weight of these bars would bend them down so as to render them unfit for action. The slips are covered with sheet iron, that they may not be injured by friction. The front ends of the bars are furnished with claws, which keep them in their places upon the slips.

Two supporters of oak, 29 feet 8 inches long each, 6 inches broad, and 4 thick, are extended from *r s* near the pinions, to *r s* at the back. These are made convex at the top so as to fit the concavity of the iron rollers A B, fig. 34. They are also covered with pretty thick sheet iron, to prevent their being worn by the motion of the weight which is to go upon them. The distance from the centre of one to that of the other is 5 feet $4\frac{1}{2}$ inches.

These things being arranged as has been described, it appears clearly that, when the handle of the first pinion is turned, the system of wheels and pinions in the machine will draw the bars, and consequently the point of support of the tube, forward into any required situation; and return it back to its former place, by turning the same handle a different way.

At S′, fig. 3. near the platform of boards, *t t v v*, is placed a barrel, 19 inches in diameter, and 17 broad, with high sides to confine the long rope which draws up the point of suspension

of the telescope by means of the pulleys that have been described. On one side of the barrel is a wheel, 2 feet 3 inches in diameter, with 91 teeth; and a handle with a pinion of 4 leaves gives motion to it, when the telescope is to be lifted up or let down.

The method of stringing the pulleys is expressed in fig. 37. The rope A, coming from the great barrel, passes successively over the pulleys 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11; and from B goes to another barrel, T′, fig. 3. which is also near the platform *t t v v*, the use of which will be explained hereafter.

By the assistance of these two motions, the telescope may be set to any altitude, up to the very zenith; and in order to have the direction of it at command, a foot quadrant of Mr. BIRD's is fixed at the west side of the tube, near the end of it, inclosed in an iron case; upon the top of which is also planted a finder, or night-glass, about 21 inches long, with cross wires in the focus. The divisions of the quadrant are indicated by a spirit-level, instead of a plumb-line.

The axle, which turns the first pinion of the mechanism for moving the point of support, carries a pallet. This gives motion to a small wheel with studs, contained in a machine fixed to the frame of the great wheel-work, and inclosed in a little box. The wheel with the studs carries a perpetual screw, which moves a central wheel, upon the axis of which is fixed an index-hand, that passes over a graduated plate of 140 divisions. Each of these divisions answers to four turns of the handle; and they are large enough that a 4th part of one of them may be distinguished. In this manner the hand will point out how many turns of the handle have been made to move the telescope from its most backward point of support to the most forward.

I call this machine the *bar-index.* It is of eminent use in giving us immediately, by means of a table made for that purpose, the place of the point of support for any given altitude or zenith distance of the quadrant.

In order to come at every part of the heavens, the vertical motion of the telescope requires the addition of the horizontal one. This has been obtained by another very simple mechanism. We have already seen that the bottom frame rests upon 20 concentric rollers, and is moveable upon a pivot.

At *w w x x y,* fig. 3. is a machine in every respect like that which has been described as giving motion to the point of support, except that instead of a bar with two lantern pinions, the great wheel here carries an iron barrel, 2 feet 8 inches long, and 5 inches in diameter. Near the ends of the great cross beam I I, are planted two pulleys; one at T, the other at V. Round the outer circular wall is a gravel-walk, 12 feet broad; and on a grass-plot close to the margin of this walk are eight posts of oak, in large frames, firmly buried in the ground, at equal distances, so as only to shew their heads sufficiently to admit an iron ring and pulley to be hung upon them as occasion may require; the middle beam also carries an iron loop at each end.

A strong rope is now thrown round one of the spokes of the wheel, next to the barrel, which passes with one of its ends under the bottom of it, while the other remains at the top. As soon as the handle puts the wheel-work in motion, the barrel will draw both ends of the rope, but in contrary directions. One of the ends is then to be led to the pulley on the great beam at T, while the other is made to pass over that at V; but in a contrary direction. Upon the nearest post at rectangles

to the great beam, towards the south, for instance, is hung a ring with a pulley to it; while, at the same time, a similar ring and pulley is fastened to another post, near the opposite end of the beam, but situated towards the north. The ends of the rope are now returned through these pulleys, and with iron hooks, which are fastened to them, are hung in the loops at their respective ends of the middle beam.

As soon as the ropes are sufficiently and equally stretched, the telescope will begin its horizontal motion, which may be continued as long as the same posts will be conveniently situated. In order to go on with the motion, the ropes are to be slackened, and the rings being then hung upon the two next posts, we may continue at pleasure to turn the telescope to any part of the heavens that may be required. The arrangement is represented upon a small scale in fig. 38.

It should be noticed, that the ends of the rope must be equally stretched, for which reason a mark ought to be made in the place, which is to be thrown round the spoke of the wheel. The fastenings of the pulleys also, which are joined to the rings that are thrown over the posts, ought to have an adjustment by links and hooks, to be either lengthened or shortened at pleasure.

With the assistance of the motions that have now been described, I have in the year 1789, many times taken up Saturn, 2 or 3 hours before its meridian passage, and kept it in view with the greatest facility, till 2 or 3 hours after the passage; a single person being able, very conveniently, to continue both the horizontal and vertical motions, at the command of the observer. In this however ought to be included an assisting third motion, which I am in the next place to explain.

We have seen that in fixing the ladders they were set at 8 feet 2 inches distance in front, in order to permit the telescope to have a side motion, without displacing the whole apparatus, which is designed for a meridional situation.

Every celestial object, when it passes the meridian, is then in its most favourable situation for being viewed, on account of the greater purity of the atmosphere in high altitudes. The advantage also of being able to direct the instrument, by means of the quadrant, to the spot in which we are to view the object, is considerable, in so large an instrument as the 40-feet telescope. With unknown objects, it is likewise of the greatest consequence to be enabled, by a meridional situation, to ascertain their place. But, as a single passage through the field of view, especially with my examinations of the heavens in zones, would not have been sufficient to satisfy the curiosity of an observer, when a new object presented itself, it became necessary to contrive a method to lengthen this interval. The tube, therefore, as we have seen, is made to rest with the point of support in a pivot, which permits it to be turned sideways.

Its diameter being 4 feet 10 inches, and that part which is generally opposite the ladders that confine it in front being about 35 feet from the pivot, it appears that a motion of 3 feet 4 inches may be had, which to the radius of 35 feet gives upwards of five degrees of a great circle.

Several abatements must be made on account of the disposition of the apparatus that gives this side motion, and the shortness of the ropes in high altitudes; but there remains, notwithstanding, a sufficient quantity of this lateral motion to

answer the purpose of viewing, pretty minutely, every object that passes the meridian.

Before I can give the particulars of this side motion, some other things must be explained. The point of support rests in a pivot; but this alone could not have given steadiness to a tube of 4 feet 10 inches in diameter, loaded with the weight of the strengthening bars, and speculum, which rest upon it. Two moveable supporters have therefore been provided at *pq*, fig. 34. They consist of two solid brass rollers, 3 inches thick, and $4\frac{1}{2}$ in diameter; set in strong frames firmly united to the sides of the tube, and resting upon the flat face of the square axle AB, which carries the pivot in the centre. The middle of these rollers is applied about 2 feet 2 inches from the centre of the pivot; and being set so as to lose none of the motion which they may have upon the axle, we find that there is room for full as much angular motion of the rollers upon the axle, as there is for the tube between the sides of the ladders; and indeed more than can be wanted, as 10 minutes of time are generally sufficient for viewing any object.

The method of observing with this telescope is by what I have called the front view; and the size of the instrument being such as would permit its being loaded with a seat, there is a very convenient one fixed to the end of it. The foot-board or floor, is 3 feet broad, and 2 feet $2\frac{1}{2}$ inches deep. The seat is moveable from the height of 1 foot 7 inches to 2 feet 7 inches, not so much for the accommodation of different observers, as for the alteration which is required at different altitudes, and which amounts to nearly 12 inches. One half of the seat falls down, to open an entrance at the back; and being

inclosed at the front and sides, a bar which shuts up the back after the observer is in his place, secures the whole in such a manner as to render it perfectly safe and convenient.

There are two strong iron quadrants with teeth, at the sides of the seat, in which run two pinions fixed upon a bar, with a ratchet and handle at the end of it. By turning that handle, the seat is easily brought to an horizontal position, before the observer enters it; or restored to it, when any considerable alteration in the altitude of the telescope renders a change necessary.

The focus of the object mirror, by its proper adjustment, is brought down to about 4 inches from the lower side of the mouth of the tube, and comes forward into the air. By this arrangement, there is room given for that part of the head, which is above the eye, not to interfere much with the rays that go from the object to the mirror; the aperture of the speculum being 4 feet, while the diameter of the tube is 4 feet 10 inches; especially as we suppose a night observer will prefer some kind of warm cap to a hat, the rim of which might obstruct a few of the entering rays.

A long coarse screw-bar is confined in a collet, which takes on and off, and may readily be put to the inclosing right side of the seat, so as to present the observer with a short handle. The other end of this bar passes into a nut, which, like the collet, moves upon a double swivel, so as to admit of every motion. The nut is planted upon a machine which will be described hereafter, and may be drawn up to any altitude, so as to bring the nut upon a level with the swivel of the handle. Upon turning the handle, the observer will screw himself, the seat, and the telescope, from the ladder; and may thus follow

the object he wishes to pursue in its course, for as many minutes as may be convenient. If, indeed, he is inclined to give up the meridian for some time, he may order the whole frame to be moved by the great round motion, which ought to be in readiness; and may even keep his object in view, as I have often done, by screwing the telescope backwards as fast as the round motion advances it. Then screwing himself forward again, he may repeat these successive motions as long as he pleases.

In those observations, which I have called sweeps (from the method of oscillating or sweeping over an arch, which at first I had adopted in the way of right ascension, but which in the year 1783 I reduced to a systematical method of sweeping over zones of polar distance), several conveniences are required: the principal of them are as follows.

An assistant, provided with an apparatus for writing down observations; with catalogues of stars, atlasses, and other resources of that kind.

A small apartment, as near to the observer as possible, in which this apparatus, with candles and other conveniences, may be inclosed.

A sidereal time-piece.

A right ascension apparatus.

A polar distance apparatus.

A polar distance clock.

A zoned catalogue of the stars.

And a ready communication between the observer and assistant, both ways.

There is also wanting, a person to give the required motions for sweeping the zones of the heavens.

A micrometer-motion to perform the sweeps.

A zone-piece, to point out the required limits of the intended zones.

A small apartment to inclose these motions, and the candle which is required for the workman.

And a ready communication, for the observer to direct the workman in the required motions.

All these conveniences were gradually brought to perfection with my 20-feet telescope; but here, they were at once, and with great advantage, designed and executed in their most improved state.

A′ B′ C′ D′, fig. 3. is the floor of the observatory, 8 feet 5 inches by 5 feet 5. It is of a proper height, and has a double window towards the west, with a shutter to be used at night. Fig. 1. gives a sufficient view of it.

E′ F′ G′ H′, is the floor of the working room, which is 6 feet 6 by 4 feet 5; and has two small windows, one to the south, the other to the east. Its height is considerably less than that of the observatory; and a view of this may also be seen in fig. 1.

The distance between the observatory and the end of the telescope, is evidently too far for a conversation in the open air, between the observer and assistant; especially as the latter, on account of his candles, must be inclosed; and ought not to leave his post at the time-piece and writing-desk. Add to this, that when the observer is elevated 30 or 40 feet above the assistant, a moderate breeze will carry away the sound of his voice very forcibly. A speaking-pipe was therefore necessary, to convey the communications of the observer to their destination.

At the opening of the telescope, near the place of the eye-glass, is the end of a tin pipe, into which at the time of observation a mouth-piece may be put, which can be adapted, by drawing out, or turning sideways, so as conveniently to come to the mouth of the observer, while his eye is at the glass. This pipe is 1½ inch in diameter, and runs down to the bottom of the telescope, to which it is held by proper hooks, that go into the tube, and are screwed fast at the inside. When it is arrived as near to the axle AB, fig. 34. as convenient, it goes into a turning joint; thence into a drawing tube, and out of this into another turning joint; from which it proceeds by a set of sliding tubes towards the front of the foundation timber.

The mechanism of the first turning joint and short sliding tube, as well as the next turning joint, is executed in brass, as represented in fig. 39. The tube *a* is the continuation of the pipe which comes down from the observer; at *b* and *c* it is turned about in an angle, but the part *b* and *c* consists of a double brass tube, one of which may be turned within the other. *b d e*, is an arm which has two pivots, one at *b*, the other at *c*; the part *d* is put through, and pinned to a fastening at the tube, where it is also permitted to turn about if required. When the telescope is lifted up, the pipe *a b c* turns upon the pivot *b*, and within the pipe *b c e*; which also turns upon the pivot *c*; so that *a b c* may come at rectangles to *b c e*, when the telescope is turned up to the zenith. At the same time *c e* sliding in *f g*, will be drawn out, since *b c* is not in the axis of the vertical motion, which lies in AB, fig. 34. but turns in a small arch about it. The point *c* will not only be drawn back, but will also be lifted up, and therefore a second turning joint, *b g*, becomes necessary, which is of the

same nature with the first. From *b i* the pipes are continued in three joints of 9 feet 6 inches long each. These slide into one another as far as is required, and, all together, into a fourth pipe, when the telescope is advanced to the place where it rests in zenith observations. The fourth pipe, which is the largest, goes to the end of the frame H′, fig. 3. where it turns towards I′; and is there again made to return to K′. At this last place it divides itself into two branches, one going into the observatory, to L′, where it rises up through the floor; the other going into the work-room to M′, where it also ascends through the floor, up to the level of the workman's ear, who stands just by the place where it terminates in the usual shape of speaking pipes. Notwithstanding the passage of the sound through a pipe with many inflections, and not less than 115 feet in length, I find that it requires no particular exertion to be very well understood; and that the communication is quite sufficient for the purpose; though undoubtedly some advantage might have been gained if brass sliding tubes had been used throughout the whole length. Under the long pipes that slide into one another, is a semicircular gutter, extended from N′ to O′, which keeps the pipes in their place, as they are carried along by the motion of the telescope, when the point of support is advanced or drawn back; and the large gathering pipe is inclosed in a box, N′ H′, to secure it from accidents.

The right ascension apparatus is constructed thus. Against the sides of the tube, and 2 feet 6 inches from the mouth of it, are fixed the centres of two rubbing plates, 3 feet 10 inches long, 2 feet 1 inch broad, and near 2 tenths of an inch thick. These plates are fastened to the long bars of the tube, nearest

the top and bottom, by six arms each; and screwed on so as to be perpendicular to the horizon. The plate on the west is fixed, but that on the east is adjustable, in order to be kept perfectly vertical on every part of its surface. One of these is visible against the tube, in fig. 1. An iron roller, 1 inch thick, and $2\frac{1}{2}$ in diameter, is set in a strong frame, in such a manner as to allow the claw, which holds it, to be set to any direction; where it can be afterwards fastened by a large horned nut. This roller is mounted upon a frame, see fig. 40. that may be drawn up to any altitude, and lies upon the whole set of ladders on the east; where it rolls up and down on six sets of brass rollers, *a b c d e f*, which are constructed as in fig. 17. This machine consists of a bottom frame, and a bar, *g b*, at rectangles to it, which, when the frame lies upon its rollers on the ladders, stands also at rectangles to them, on the lowest part of the frame: it is braced so as to make the greatest resistance from east to west. The bar carries the iron roller, which may be shifted to two different situations; *g* almost down to the ladders, and *b* more elevated. The latter is used in high altitudes. The iron roller, standing out, is then turned so about as to be in the direction of the length of the eastern rubbing iron; in which situation it is fixed by the horned nut. The telescope is then brought forward, or backward, by the bar machine, till the rubbing iron comes to be opposite the roller.

Upon one of the braces of this same frame at *i*, is also planted the nut belonging to the lateral screw motion, which has been described; and its long bar goes always with this machine, when it is disengaged from the observing chair, and is laid back at *k* into a secure resting place.

On the opposite set of ladders is another machine, which

carries a large spring-bolt, on the end of which is mounted an iron roller, exactly like that which has been described. This is also adjustable to any direction. The bolt is contrived in such a manner as to come out of the frame, in which it runs, with a pressure of 34 pounds; and it exerts, very nearly, the same force during the time in which it goes through every part of the space it describes. The construction of the springs is expressed in fig. 41: *a b c* are two iron bars, 5 feet 6 inches long; jointed at *b*, like a pair of compasses, and fastened on a pivot at *a*, which remains immoveable upon the frame, while the other end is also fastened on a pivot, fixed to the bolt, which carries the roller *f*. The bolt is 7 feet 1 inch long, and 3 inches square. It runs in two sets of four brass rollers each, at *g* and *h*, which embrace it completely, and prevent friction as much as possible. The joint *b* is sustained by a brass roller, which runs on the iron plate *i k*. Two tapering steel bars, or springs, *l m*, *n o*, are fastened against the lower ends *a c* of the iron bars; one of them is convex at *m*, the other concave at *o*; and they exert their force against each other at *m o*, where the convex one rests in the concavity of the other. There is an adjustment of the flap which carries the bolt, see fig. 1. by which it may be raised up, so as to become exactly opposite to the roller on the east, when that is raised to its highest position.

It will now be easily perceived, that when the eastern rubbing plate, in its well adjusted vertical position, is pressed against the right ascension roller, by a roller exactly opposite, and with a force sufficient to keep it firmly poised against that roller, a vertical motion may be given to the telescope, in

which the same meridional situation will be preserved. Accordingly, I find that the right ascension of unknown objects, deduced from known ones, observed by the same instrument, and in the same zone, is capable of great precision; and this construction will therefore answer all the ends that were proposed. For it would not be doing justice to the telescope to require of it all the accuracy of a transit instrument.

The spring-bolt, as I call this latter machine, is brought to any required situation by a rope fastened to the middle cross-beam of the stand, which comes down, and goes through a pulley placed upon the machine; in its return to the top, it passes over a second pulley, and then goes down to a barrel with a wheel and pinion, on the ground timber at Q′.

The polar distance machine, as I call the opposite one, on account of its chief use, which remains still to be explained, is drawn up and down in a similar manner, by the handle of a pinion, wheel, and barrel placed at R′.

In the observatory is placed a valuable sidereal time-piece, made by Mr. SHELTON, for which I am obliged to my astronomical friend Mr. AUBERT, as a gift that will always be highly esteemed. Close to it, and of the same height, is a polar distance piece, which has a dial-plate of the same dimensions with the time-piece; and is also divided into sixty parts on the outside; but these are to express minutes of space. Every tenth is marked with large figures, but every single one is also denoted with its proper figure, in a smaller character. The degrees are shewn in a square opening under the centre, and change backwards and forwards as the telescope rises or falls. This piece may be made to shew polar distance, zenith dis-

tance, declination, or altitude, by setting it differently; but in conformity with FLAMSTEED's British catalogue of stars, I have generally adopted polar distance.

The construction of this piece is very simple. It contains only one barrel, for the weight and line, which gives motion to the work; and two small index wheels. The line is conducted from the polar distance machine into the observatory at the bottom of the polar distance clock, where it rises up, and passes over the barrel. By making this revolve, it moves the hand upon the axle of it, which points out the minutes upon the dial-plate. The hand is made adjustable in the usual way of the minute hand of common clocks, by going upon a pipe, kept firm by springs. The line is of considerable length; but the case of the clock being no larger than that of the time-piece, a set of neat and very thin pulleys, four and four, are used to draw the end, after its having crossed the barrel. It is necessary to mind, in setting these pulleys, that they should run upon very thin pivots, and clear one another perfectly; as otherwise their action might not be adequate to the purpose; this however is only to stretch the end of the line freely and sufficiently, that in passing over the barrel it may not make it turn about irregularly. There will be no occasion for a revolution of the line upon the barrel, as I have found a mere passage over it of sufficient effect in turning it; for the hands must all be properly counterpoised. Each revolution answers to one degree of change in polar distance; the minutes are therefore pointed out by the hand it carries. The two small index-plates I have mentioned, are fastened upon pivots against the back of the dial-plate, between it and the frame of the barrel. They are placed so, that their edges meet not far

from the centre of the square hole, I have mentioned, in the dial-plate, for shewing the degrees; and a small square portion, a little more of one than the other of the two wheels, may therefore be seen, in front of the dial-plate, through the opening in it.

These wheels carry contrate teeth on the inside, and a small dial-plate on the back. The face of the dial-plate of the wheel which presents itself at the right, carries the units of the degrees; 1, 2, 3, 4, 5, 6, 7, 8, 9, 0; while that on the left has a blank which remains till the 0 of the first appears. Upon the axle of the barrel, close to the frame-plate on the outside, is fixed a long counterpoised contrate pallet; which at every revolution sweeps over one of the teeth of the first wheel, of which there are ten. The shape of the pallet must be like the barb of an arrow; but more obtuse, that it may take as much time in entering very obliquely into the teeth as possible, to avoid a sudden shock. The movement will even then be found to be quite quick enough, for shewing almost instantly the proper degree of polar distance. But to counteract the sudden stroke of the long pallet, there is over each wheel a small lever, see *a b*, fig. 42. that rests with its end between the two uppermost teeth *c d*; and its shape is that of a very obtuse angle, such as 160 degrees. The point of the angle sinking down between the two teeth, by its slope both ways, prevents their overshooting. The lever is held down with a very weak spring, *e f*, the point of which touches the lever at *e*, near the place of its pivot. This method will even throw back the figure upon the dial, if it should have been overshot a little. Care must be taken to let all this work be light, that no great force may be required in the long pallet to move it.

The first wheel in turning about carries a short pallet, of a shape similar to the long one. This must be placed low enough to let the long pallet pass freely, and high enough to clear the spring-lever in going over it. The pallet, on the appearance of the 0, strikes a tooth of the second wheel, and brings the figure 1 into view, which with the other forms 10. The second dial-plate has a blank, and the figures 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, engraved upon its face, and presents thirteen teeth to the pallet on the first wheel, by which the blank and figures are successively brought into view, along with the succession of the units on the dial-plate of the first wheel.

In this manner the degrees are shewn from 0 to 129, which includes the whole range of north polar distance in this latitude; while at the same time they are properly subdivided into minutes. A more minute division was not thought necessary with this instrument, and indeed ought not to be aimed at.

The cord which gives motion to the polar distance clock, is rendered a just representative, or true index, of the angular movement of the telescope in the following manner.

On the machine which holds the right ascension roller, is an arm *l m n,* fig. 40. in an oblique direction, upon which is fastened a brass slider, 3 feet 1 inch long. A coarse screw passes from one end of it to the other, and is confined between its shoulders *l n.* At *o* there is a handle, by which the screw being turned, a small sliding plate *m,* which carries a pulley, is drawn backwards or forwards at pleasure, along the whole range of the slider.

On the telescope, near the bottom of the front edge of the

eastern rubbing-plate, is a small square bar with a loop upon it, which is adjustable, so that it may be occasionally brought a little nearer to the mouth of the telescope, or removed farther from it. The end of the polar distance cord is fastened to the loop upon this bar, where it remains when the polar distance clock is not in use. By this means the weight which stretches it in all its length from the telescope to where it is suspended in the clock-case, is kept always equally exerted, and no relaxation of the cord, which ought to be avoided, can take place.

When the polar distance clock is to be used, the cord is lifted into the pulley of the slider at *m*, and now goes from thence to its destination as before. The right ascension roller resting against the rubbing plate, the pulley of the slider is near at hand, and the cord may easily be lifted into it. The handle *o* is now to be turned till the cord, which goes from the loop at the telescope to the pulley upon the slider comes to cover a certain white line or mark upon the side of the tube. This line when it is first made, must be placed so as to be vertical when the radius of motion of the loop is a little more than one degree of elevation above the horizon.

The theory of this arrangement is, that when a motion in polar distance takes place, the tangent and the arch may be looked upon as equal for a few degrees, in a mechanism which aims only at minutes. And, indeed, as far as two degrees and twenty minutes, when the motion is taken equally both ways of the adjusting point, the deviation from truth will not even amount to quite one second.

The cord from the pulley of the polar distance machine passes

straight away to O′, fig. 3. where it is bent over a small pulley to one just close to it, which leads it in a direct line to P′, under the polar distance clock, where it rises up to the barrel.

The barrel is of such a diameter as to answer as nearly as possible to the length of the cord which is drawn by the motion of the telescope over one degree of polar distance; but as the utmost accuracy could not have been obtained in the make of the barrel, the loop at the telescope which draws the end of the cord, as we have described, may be slipped backwards or forward upon its bar, which will either lengthen or shorten the radius of its motion, and occasion its drawing more or less of the cord.

As there is a good quadrant upon the telescope, there remains nothing else to obtain a just position of this loop than to compare the indication of the polar distance piece with that of the quadrant; and when the former is regulated to a perfect agreement with the latter, we may safely rely upon the truth of its report.

The time and polar distance pieces are placed so that the assistant sits before them at a table, with the speaking-pipe rising between them; and in this manner observations may be written down very conveniently. The place of new objects also may directly be noted, as their right ascension and polar distance is before the assistant upon the table, where nothing is required but to read them off, on the signal of the observer.

By a catalogue in zones the assistant may guide the observer, who is with his back to the objects he views, and who ought to have notice given him of such stars as have their places well settled, in order to deduce from their appearance the situations of other objects that may occur in the course of

a sweep. In the year 1783, when I began this kind of observations, no catalogue of stars in zones had ever been published; I therefore gave a pattern to my indefatigable assistant, CAROLINA HERSCHEL, who brought all the British catalogue into zones of one degree each, from the 45th degree of north polar distance down to the horizon, and reduced the right ascension of the stars in it to time, in order to facilitate observations by the clock. This catalogue was afterwards completed from the same degree up to the pole in zones of 5 degrees each; and the variation in right ascension from one degree of change in longitude, was also reduced to time, for every star in the catalogue. To this were added computed tables for carrying back present observations to the time of that catalogue; which method I preferred to bringing the stars it contains forward to the present time, on account of conforming with the construction of the Atlas Cœlestis, which was of great service.

The evident use of such a catalogue must undoubtedly soon have been perceived by every person who was acquainted with the method I used for sweeping the heavens; and as the same is practicable, not only with my telescopes, but likewise with transit instruments, and mural quadrants, we are now much indebted to the Rev. Mr. WOLLASTON, who in the year 1789 favoured the astronomical world with a work of nearly a similar construction with that which I was in the habit of using; but much enlarged, and enriched with stars taken from the best authors; and moreover reduced to the time of the year 1790.

We now seem only to want an atlas on the same construction, upon a scale equally extensive, and plentifully stored with well ascertained objects.

The micrometer-motion which is required for sweeping the heavens, and indeed for viewing the planets or other objects, is obtained by means of the end of the rope B, fig. 37. which draws up the telescope. This goes down to a barrel, S′, fig. 3. 12 inches long, and 4 in diameter, joined upon the same axle with another barrel, 12 inches long, and 12 in diameter. A smaller rope goes from the largest barrel into the working-room, where it is fastened to the top of a thin vertical spindle, 2 feet 6 inches long, and 3 inches in diameter, at *a*, fig. 43. Another rope of equal size is fastened to the bottom of the same spindle at *b*; and when by turning the handle *c d* the rope *a e* is wound upon the spindle one way, the rope *b f* is wound off the contrary way. This second rope *b f* goes out of the work-room over a pulley, which leads it upwards to the top of the middle cross beam of the ladders, where it descends over another pulley, by a weight with shifters which is suspended to the end of it. In this manner a balance is obtained between the stress of the ropes *a e* and *b f*, which leaves the spindle at rest in any position where it may chance to stand, and considerably eases the labour of the workman, who turns this handle a certain number of times one way, and then the same number of times back again. By such a motion of the handle the telescope is alternately depressed and elevated; and this being continued for as long a time as the observer chooses, enables him to review the heavens as they pass by the telescope.

By the arrangement of the barrels, it is easy to see that the motion is sufficiently divided; as many turns of the handle are wanted to pass over a small space of the heavens. The method of barrels and ropes is to be preferred to wheel-work, on account of the smoothness as well as silence of the motion,

both which in observations of this kind are highly necessary. It is true that the great stress which lies on the ropes of the micrometer-motions wears them out very fast, and they must therefore be carefully watched, and often renewed; but this ought to be no objection where the end to be obtained is of such consequence.

It would not only be troublesome to the workman, but often bring on mistakes, were he to count the turns of the handle, which perhaps for hours together he is moving; a zone-clock, therefore, has been contrived to release him from that care. This is a machine which is placed upon a table just by the workman. It strikes a bell when he is no longer to turn one way; that is, when the telescope is come to one of the limits of the zone, which if it be after going down, is called the bottom bell; and it strikes another bell when he has made the same number of turns in a contrary direction. The telescope is by this motion restored to its former situation, and this second notice is called the top bell; which marks out the other limit of the zone. These bells not only give notice to the workman when he is to change, but their different sound indicates the position of the telescope, and prevents mistakes.

An additional precaution has been used, to make the bells repeat their stroke, the very next turn, if by some mistake the workman should have been inattentive to the first notice. In a long continuation of uniform intervals of sound, we may become so used to them as hardly to perceive them at all; but the coming in of an additional sound will immediately rouze the proper attention. Another very necessary use which I have often made of a second or third bell, is to extend the zone, either towards the north or south, for some time, when

notice has been given of a star that was a little above or below the sweep; for in some parts of the heavens known stars are scarce, and it becomes necessary to take in all those that may be come at.

The construction of the zone-clock is very simple and convenient. The end of the axle which holds the double barrel, fig. 3. must be left projecting at T′. Upon this a small hollow cylindrical pipe is placed, which holds the end of the cord that is to move the zone-clock. The pipe must be guarded at both ends like a clock barrel, to keep on the cord, but remain open at the end which goes upon the axle, upon which it must fit upon a square so as to keep firm. It should be about 1¼ inches long, and 1 in diameter.

From this piece the cord is made to pass to the work-room, where it rises up into the clock at *a*, fig. 44. It then passes over a large narrow barrel, *b c d*, and by means of a weight *w* at its end, descends when the handle of the micrometer-motion turns the spindle and double barrel with which the pipe that holds this cord is connected. At *c b* are two levers that, in the usual way, occasion the hammers *e f* to strike the bells *g b*, when the pins quit the levers which they have lifted up. But these levers have spring joints, so as to permit the pins to pass back again without disordering the work. The pins which move the lever *c* are fastened to the barrel *b c d*. The lever *b* must be brought out so as to be before the front of the first frame-plate, and close to a dial-plate, which is to contain about 40 numbers. The dial-plate must be pretty thick, and be fixed upon a hollow arbor. The axle of the barrel, which should be strong, must be long enough to come through the hollow arbor, and project a little way to receive a milled nut

upon its end, which must have a screw upon it. The arbor which carries the dial-plate is then to be pinned fast upon the axle, and an adjustable hand being put upon the projecting arbor, a collet is slipped over it, and the milled nut screws it down, in any position that is to be given to it.

The adjustable hand is made of a piece of springy iron, or steel, formed as represented at *ik*; but broader than clock hands usually are. It must have a pretty large circle in the middle, with a hole wide enough to go upon the plate-arbor. The end *k* of the hand must project beyond the dial-plate a little way, so as to permit two screws, *m n*, to pass by it into a brass plate, with a small piece between, to allow some motion up and down to the hand. The plate which is fixed to the hand by the screws *m n*, returns under the dial-plate sufficiently to carry three pins that are to lift the lever *b*, when they come to the proper situation, in the same manner as those on the barrel lift the lever *c*. The dial-plate, close to the margin, should have as many small holes, to receive a pin, as there are numbers marked upon it; and in the hand, answering to the holes, must be fixed a steady pin to fall into any one of them, when the hand comes to be placed over it. There must be a small handle near the end *l* of the hand, by which it may be lifted up, and moved into any situation that shall be required; and care must be taken to have both ends properly counterpoised.

In order to set the zone-piece to the breadth of any particular sweep, as for instance two degrees, we make the workman begin at the striking of the top bell, and while he turns the handle till the quadrant or polar distance-piece points out a change of two degrees, we keep the hand of the zone-clock lifted up, that the pin may be out of the holes upon the dial-

plate; for which purpose also the nut in the centre must be unscrewed a little to permit it to pass freely. When the telescope has descended two degrees the workman must stop the handle. We then lift the hand to the place where the first pin strikes the lever of the bottom bell. Here we let the pin drop into its proper hole, and screw fast the central nut. When this is done, the workman may turn backwards and forwards from bell to bell, and the telescope will perform the required motion of two degrees.

The work of the zone-piece is arranged in such a manner as to make the numbers on the dial-plate answer to turns of the working handle: this however, though convenient, is not absolutely necessary. The number of turns to a degree varies a little in different altitudes; but by trial a table may be made, which will shew with sufficient accuracy the figure on the dial-plate to which the hand must point, that the zone-piece may give any required breadth to a sweep, at any certain polar distance.

By means of the speaking-pipe the workman may be directed to begin, to stop, to go fast, or slow. And these, with a very few other orders, will be all that are wanted; which being known to him and to the assistant, will occasion no mistake, notwithstanding the pipes which go into the two apartments are united.

The ropes that come from the gallery, each bracket of which is separately drawn up, go through a double pulley, hung to the top cross beam, and a double pulley fastened to the upper end of the gallery bracket; after this they pass over a single pulley at the top, down to two barrels placed under the back of the ladders, one on each side. Each barrel is moved

by a handle, on the axle of which is fixed a pinion of four leaves: this works in a wheel, on one side of the barrel, of 61 teeth, and 18 inches in diameter.

The barrels are $25\frac{1}{2}$ inches long, and 12 inches in diameter, that the rope may not be doubled often, which might hurt the uniformity of drawing up the gallery. They are made exactly alike, and draw an equal length of rope at every stroke of the handle; but as one of the persons who draw the gallery might go on quicker than the other, each of the handles strikes a bell at every turn, going up as well as going down; the different tone of the bells easily shews, by sounding in regular alternate succession, when the gallery is properly moved; which therefore may be safely done in the dark. The mechanism of the bell-work at each handle is in a little box, to keep it dry, but sufficiently open at the side to throw out the sound.

A single bell being suspended, as in fig. 45. upon a plate of iron, at *a*, there is a cock, *b c*, planted upon it; between which, at *d* and *e*, are inserted two axles continued outwards. On the outside of the cock, and upon these axles, are two inverted hammers suspended, with lever arms, *f g*. These are made with spring joints, like those that have been described in the zone-clock. The axle which moves the barrel has a pallet upon it, and the plate with the bell apparatus being presented at rectangles to the axle, so as to make the pallet play in the notch of the plate between *f* and *g*, where the lever arms meet, it will make the bell give a stroke, either with one hammer going up, or with the other coming down.

It is necessary to preserve the pliability of the ropes, for which reason no tar has been used with any of them that are about the telescope. To preserve them, however, they are

passed through very hot melted tallow, and kept a sufficient time immersed in it, that they may be thoroughly penetrated. In this state they will last a considerable time, especially when care is taken not to relax them often. The gallery being suspended by ropes in this state, it would be unpleasant to trust entirely to them. Each bracket therefore is furnished with four strong broad iron hooks, two of which take hold of one of the flats of the division $\beta\gamma$, fig. 7. while on the opposite side two more take hold of the corresponding flat of $\delta\epsilon$. When the gallery has been drawn up to the required altitude, the hooks are let down, and the ropes slackened a little, so as to permit it to hang in the hooks. The other two hooks on each side serve for an elevation between the flats half way from one to the other. They are upon the same centre with the former, and fall back as the others do when the gallery is to go down.

For the safety of the tube also, there is a strong chain, which will sustain it, in case the ropes by which it is suspended should give way. This is fastened into a loop near the point of suspension. The other end of it is hooked upon a flat, and passes round one of the side beams of the ladder at a certain elevation above the telescope, and is sufficiently long to permit the tube to move a few inches more than is necessary. By this means a fall can never be considerable: if the ropes were to break in the worst part of a sweep of $2\frac{1}{2}$ degrees broad, the telescope would hardly descend two feet.

The construction of the great mirror is as in fig. 46. The metal itself is $49\frac{1}{2}$ inches in diameter, but on the rim at *a b* is an offset of $\frac{3}{4}$ inch broad, and 1 inch deep, which reduces the concave face of it to a diameter of 48 inches of polished sur-

face. The thickness, which is equal in every part of it, remains now about $3\frac{1}{2}$ inches; and its weight, when it came from the cast, was 2118 pounds, of which it must have lost a small quantity in polishing.

An iron ring, $49\frac{1}{2}$ inches in diameter within, 4 inches broad, and $1\frac{1}{8}$ thick, has at the face of it on the inside a strong bead or rim added to its thickness, which fits the offset in the speculum, but is not quite so deep as that. A cross of the same substance of iron as the ring, goes over its back, and when the speculum is placed into the ring, so as to rest upon the offset, the cross over the back confines it in the ring. By the addition of a thin cover of sheet iron on the back, and another of tin on the face, the rim makes a complete case for the mirror.

Three strong handles are fixed against the sides of the ring, by which the speculum may be lifted horizontally, or using only one of them, vertically, as occasion may require.

To put the speculum into the tube, there is provided a small narrow carriage, going upon two rollers. It has upright sides, between which the speculum, when suspended vertically by a crane in the laboratory, is made to pass in at one end, and being let down, is bolted in. The carriage is then drawn out, rolling upon planks, till it comes near the back of the telescope. The tube must be put back as far as the bar-machine will permit it to go. Two beams connected together so as to form a parallelogram of 8 feet 6 inches long, and 2 feet broad, are sloped away on one end, while the other contains two hooks, by which it may be hooked into two holes at the end of the foundation timber, fig. 3. in the middle between the rolling beams *r s*. This affords a passage of an easy ascent to

the speculum carriage, which must now be brought into a proper position for rolling up. When this is done, the carriage is to be tied to the axle of the point of support, A B, fig. 34. and by turning the bar-machine handle, the speculum with its carriage will be drawn up to the foundation beams E E, A A, fig. 3. which are 16 inches above the foundation wall. By the time that the carriage comes near to the top, there will be room for six 3-inch planks that are provided, to be laid one after another upon the rolling beams *r s*, which will form a platform of 5 feet 10 inches by 5 feet 5, for the reception of the carriage. But these planks must not be put down till the telescope has been first brought back, and fixed again close to the carriage, which must be sustained in its place while this is doing. Then, advancing again, the platform is laid down, board by board, till completed, while at the same time the carriage will be drawn upon it.

As soon as that is safely landed, a strong rope is to be hooked into a loop, fixed upon the beam at *a*, fig. 15. This going down to a pulley with a swivel hook at the bottom, which is put through one of the three handles of the speculum, returns to a pulley hung upon the hook *b*. From that pulley it goes forward to a leading pulley at V′, upon the foundation timber, fig. 3. This directs the end of the rope to the barrel, which serves for the great round motion of the whole telescope. When the handle of that machine is moved, the speculum will be lifted up in its carriage, which being eased, must now be turned about while the mirror is yet partly resting upon it, so as to become parallel with the back of the tube, and close to it. As soon as the mirror is fairly suspended, the carriage must be unbolted, and drawn sideways from under it.

At the same time the platform must be gradually removed, that the tube may be brought back by the bar-motion, whenever the mirror is high enough to pass over the back of it. Then letting down gently the round motion handle, and guiding the mirror properly, it is to be placed upon a small hollow square, with a sloping back, which is planted under its support. The height of the square frame is such as will bring the centre of the mirror into the centre of the tube; and the sloping back receives it in going down, and throws it from the back of the tube, just as much as is required to make the adjustment at the top act properly.

When the mirror is in its place, two loops which are prepared are to be screwed fast to it. They contain the collets that receive the adjusting screws from the back, through the strong upper bar *c g*, fig. 34. and as soon as these are fastened the pulleys may be unhooked, and all the apparatus that has been used removed. The six planks are then to be laid upon the same rolling bars at *n o*, where a passage across the work is wanted, and where they may remain till zenith sweeps require them to be moved.

The method of preserving the speculum from damp is by having a flat cover of tin soldered upon a rim of iron, about $1\frac{3}{4}$ inches broad, and $\frac{1}{8}$ thick, the diameter of which is equal to the iron ring which holds the speculum. Upon the flat part of the rim is cemented, all around, some close-grained cloth of an equal breadth with the rim. The cover has two handles near the upper end, and under them two flaps that project about an inch and are six inches broad. When the cover is hung or laid upon the speculum, so that the two flaps are close to the ring which incloses it, the rim of the cover, as far as it

is lined with cloth, will rest against the edge of the iron ring, and fit it all around very closely.

In six places are painted white marks which divide the circumference equally; and six claw-screws are provided, of the shape that is represented in fig. 47. These are applied to the six marked places. The end *a* being put over the iron ring *b* to take hold of the back, the screw *c* is then fastened so as to press upon the outside of the cover and rim, till the lining of it is brought into close contact with the iron ring.

To take off and put on again the cover, a small ladder is provided, which being set at the outside against the back of the tube, the person who is to uncover it goes up, and descends into the tube by means of a board with steps. This board goes across the mirror in a parallel direction with it, and being narrow, does not interfere with the work of loosening the screws to take them off. When they are removed, the person comes out of the tube the same way, still leaving the speculum covered, but when at the top of the ladder brings out the inside board-steps. The two handles of the cover now present themselves at the back, so that two persons can easily lift it off, without suffering it to touch the mirror in any place. It must then immediately be carried into the observatory, and remain there till the mirror is to be covered again; but first of all the inner and outer cover of the tube ought to be carefully closed up.

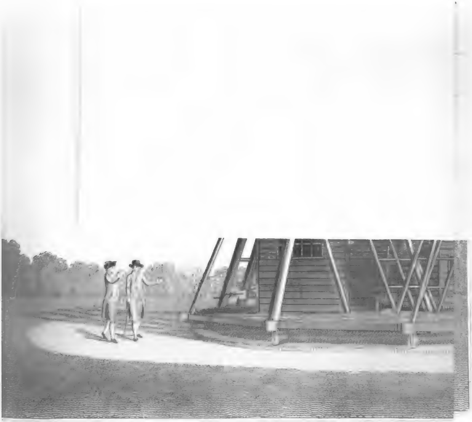
When the speculum is to be covered again, great care is required to see that no drops of dew may fall from the outer cover of the tube upon the inner one; or at least that these may not find their way to the mirror; and to let the first

object be to put its own cover upon it before any thing be done about fixing it there.

In very high observations the tube will not fall down again readily, and in the zenith, by its great weight added to that of the mirror, will even tilt backwards. A counterpoise therefore is applied by a meridional post, 7 feet high, well fastened by a frame in the ground; and placed about 20 yards from the front of the telescope. To this is fastened at the top on the back an arm, which carries a pulley, and at the bottom on the front, a barrel moved by a wheel and pinion. A rope with a weight fastened to the end of it, goes over the pulley on the post, and towards the mouth of the telescope. At the end of the tube on each side is a loop, into which a chain is hung with both ends. It is long enough to go round the seat to a considerable distance, and holds a pulley in the middle, over which the rope from the weight is made to pass back again to the barrel at the post. Here it may be drawn up till the weight is lifted sufficiently to keep the telescope steady, and to make it fall again, when its own motions lower it. In zenith sweeps 300 weight are required for that purpose, but one hundred of that quantity is in shifters that may be taken off in lower altitudes, when less is sufficient.

A similar post and apparatus is fixed about the same distance from the telescope towards the north, to be used when the instrument is turned about for high observations in the northern meridian.

Another inconvenience to be removed in very high altitudes, is that the long bars, which bring the point of support forwards, begin to project beyond their supporters. When this



TO GEORGE THE THIRD

This View of a Forty Feet Telescop

is with permission, most humbly inscribed, by t

and most grateful obedien

Fig: 2.

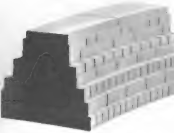
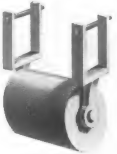


Fig: 4.



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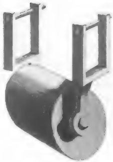

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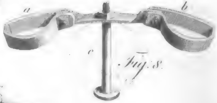

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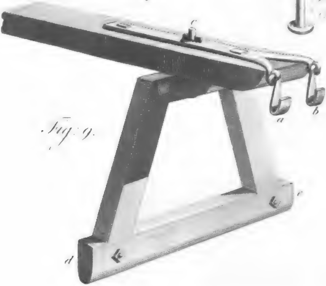
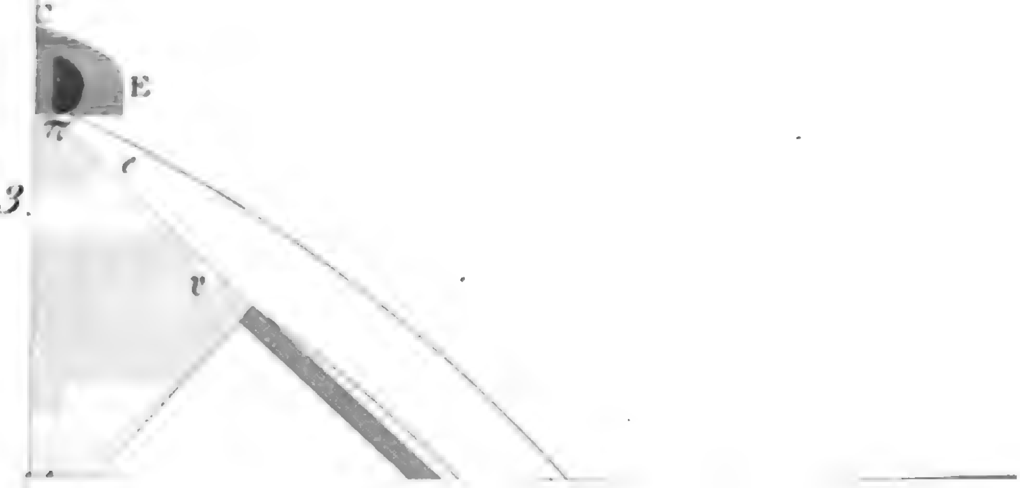


Fig: 9.

Fig: 3.



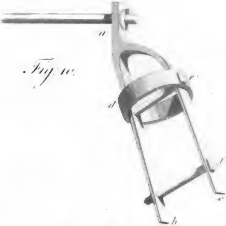


Fig. 10.

Fig. 12.

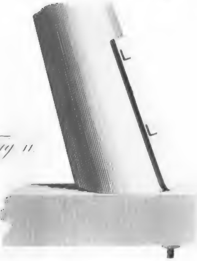


Fig. 11.

Fig. 14.

Fig. 13.

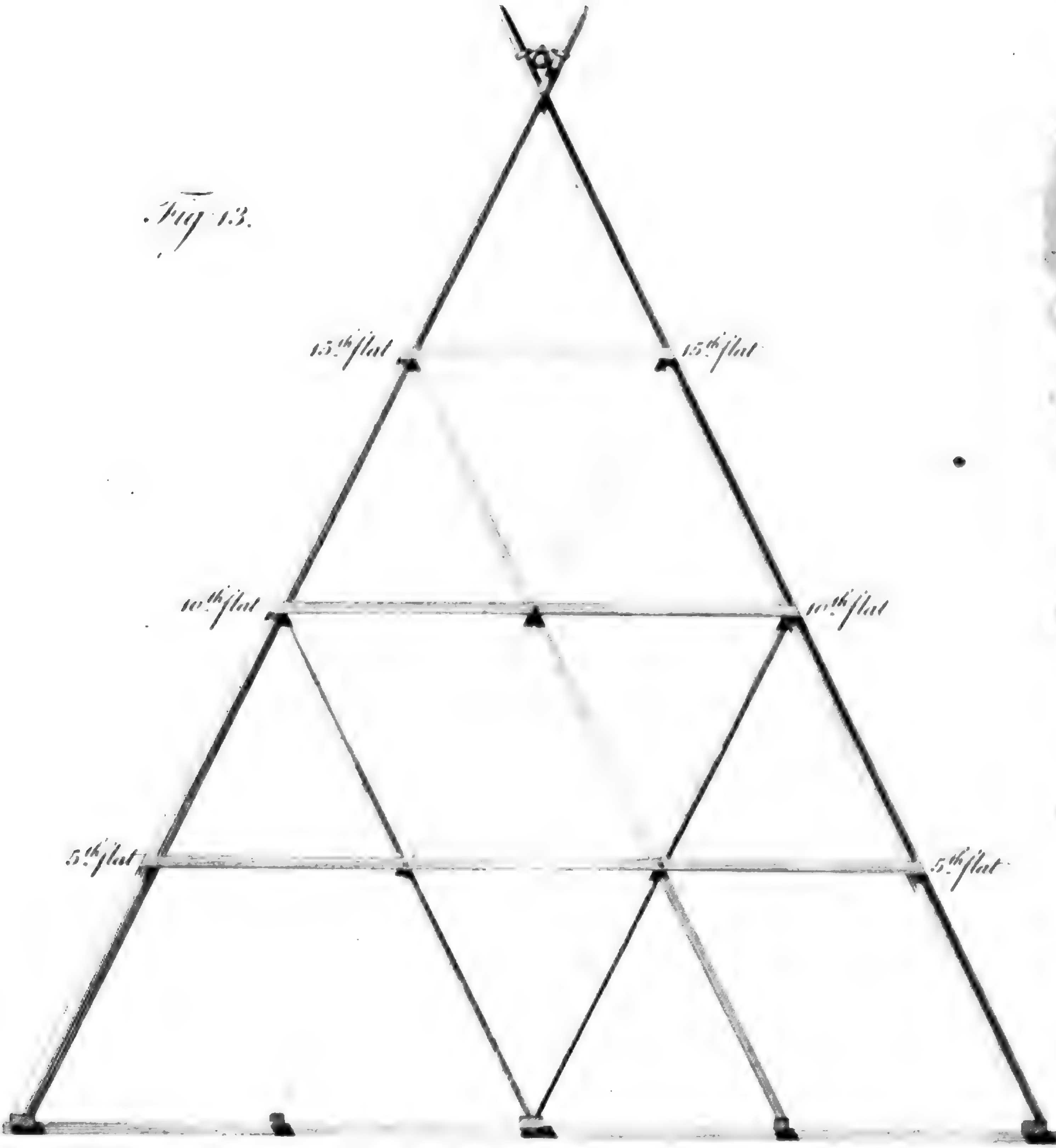


Fig. 15.

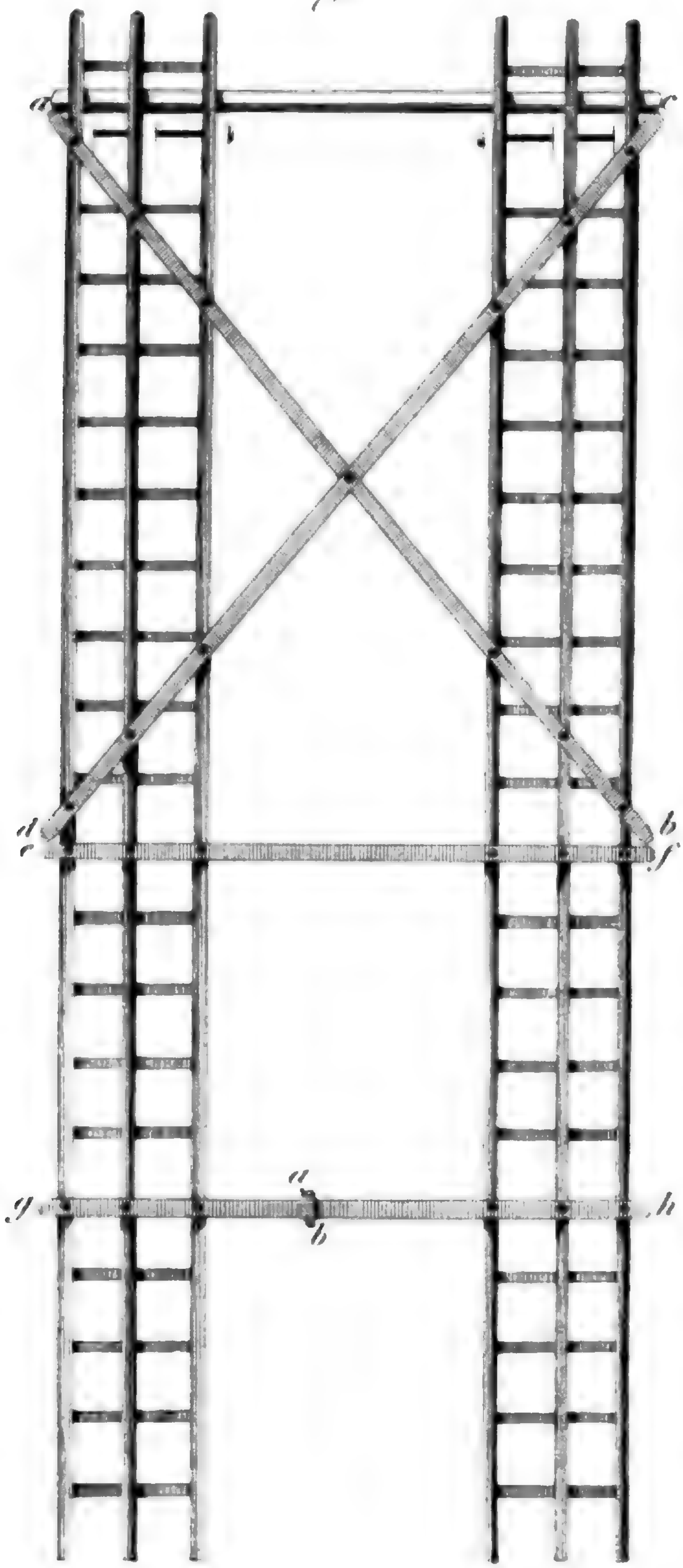


Fig. 16.

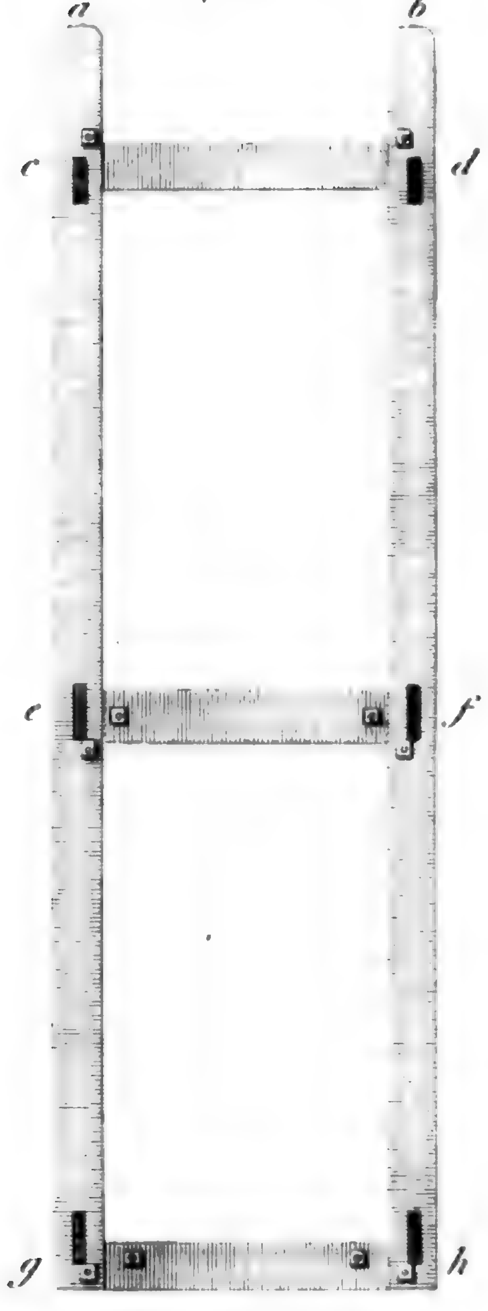


Fig. 21

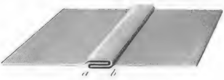


Fig. 22

Fig. 25

Fig. 23

Fig. 24

Fig. 26

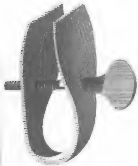


Fig. 27

Fig. 28.

Fig. 29.

Fig. 30.

Fig. 31.

Fig. 32.

Fig. 33.

Fig.

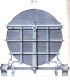


Fig. 35.

Fig. 36.

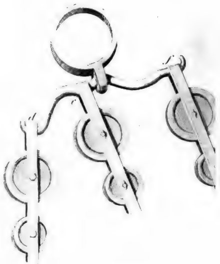


Fig. 37.

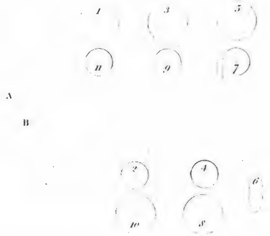


Fig. 38.

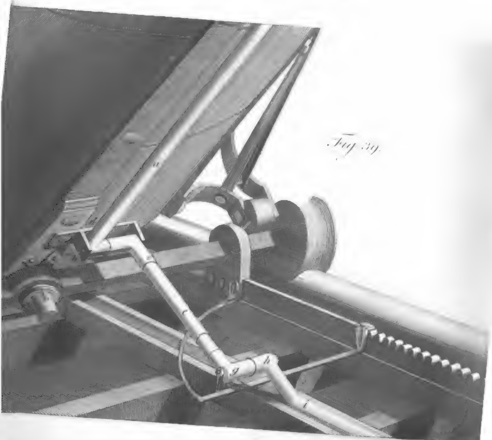


Fig. 39.

comes to take place, a light iron frame with two small wheels, or rather rollers, is pinned to the ends of them. This not only keeps them together, but also supports them sufficiently as far as they are to come out.

A slider, upon an adjustable foundation, is planted at the mouth of the telescope so as to be directed towards the centre of the mirror. It carries a brass tube, into which all the single eye-glasses, or micrometers, are made to slide. When they are nearly brought to the focus, a milled head under the end of the tube turns a bar, the motion of which adjusts them completely.

The focus of the great mirror is directed to its proper place, by putting two plates with springs upon the rim that limits the aperture of the tube, into two places which are marked. Then a cap with a small hole being put into the sliding tube, an assistant with a proper handle must screw in or out one or other of the adjusting screws at the back of the mirror, till the plates upon the aperture in front of the telescope become both visible; for they are contrived so that when the mirror is not properly adjusted, either one or both will vanish. At the same time these plates, by their situation, serve to inform us which of the screws, whether that to the right or that to the left, is in fault, by which means the adjustment becomes a very easy operation.

Slough, near Windsor,
May 18, 1795.

WM. HERSCHEL.

XIX. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1794. *By* Thomas Barker, *Esq. Communicated by* Thomas White, *Esq. F. R. S.*

Read June 18, 1795.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			Lyndon.
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.
Jan.	Morn.	30,03	28,06	29,50	42	31	36	43	23	32	0,417
	Aftern.				42½	32½	36	48	27	37	
Feb.	Morn.	29,77	28,89	32	49	41	45	51	36½	43	1,396
	Aftern.				56	41	45½	56	41	48	
Mar.	Morn.	29,98	28,97	42	49½	42	45½	50	32	41	1,656
	Aftern.				50	42½	47	55	41	49½	
Apr.	Morn.	29,96	28,47	42	59	44	51	55½	36	46	1,990
	Aftern.				61½	45	53	74	41	58	
May	Morn.	30,11	28,96	52	60	49	53½	59	41	48	1,039
	Aftern.				62	51	55	71	49	59	
June	Morn.	29,88	29,23	59	65½	53	60	68	46	55½	0,708
	Aftern.				70½	54½	61½	86	53	69½	
July	Morn.	29,93	28,96	54	70	61½	66	67	56½	62	4,199
	Aftern.				73	62	68	81½	68	76	
Aug.	Morn.	29,82	28,96	44	66½	57	61	64	47	56	2,881
	Aftern.				68½	59	63½	76½	60	67½	
Sep.	Morn.	29,89	28,66	40	60½	48½	56½	60	36	50	3,573
	Aftern				63	50	57½	69	48	59½	
Oct.	Morn.	29,88	28,54	33	56	44	51	57	35½	46	3,535
	Aftern				57	45	53½	62½	42	53	
Nov.	Morn.	29,71	28,60	24	49	38½	45	52	31	41½	3,963
	Aftern.				51	39	46	55	36	45½	
Dec.	Morn.	29,92	29,01	44	50	31½	40	53	25½	36	1,219
	Aftern.				50	32	40½	53	29	38½	
June 22 to July 21		29,93	29,26	29,62	70	61½	66	68	57	62	26,576
					73	63½	68	86	68½	77	

THE year began with frost, but a broken one, neither severe nor settled, with much sun, calm weather and pleasant, yet the ice was scarce gone at the end of January; in the latter part of the month there were some strong winds, which were more frequent afterward; showery, mild, almost without frost, pleasant and forward. Several autumnal flowers continued in blow all winter; the winter ones were early, and the spring ones forward; many anemonies, which are properly a spring flower, were blowing all winter, but had not then their full colours which they have in their season. This open winter was not a wet one, which was very convenient, as fodder was scarce, and turnips late and small, but were much mended by the mild autumn. The ground was green almost all winter; and there were very few NE winds in March and April, and there was pretty good grass in that last month, which was a great advantage when a colder season came on in May. Oaks began to be cut soon after the 20th of April, and the hawthorns to blow before the end of the month.

With May began a colder season, with frequent frosty mornings, blasting the fruit after it appeared set, and also the young leaves very much, and more northerly winds, especially toward the end of the month, and the former part of June; for they seldom fail of coming sooner or later in the spring; yet the weather was often fair, fine, and pleasant, but the ground getting too dry. The latter part of June and most of July was remarkably hot, and for the most part burning; but some single large rains in July, particularly toward the end of it, prevented its burning so much here as it did in the south of England, where the drought was greater, and lasted much longer; there they suffered very much by it; but from

the forwardness of the beginning of spring we were never without grass, though it was burnt. There were good crops of hay on some of the low moist meadows, but the uplands and late laid were light. The heat of June and July, and middle of August, brought things very forward. Harvest began about July 20, and was nearly finished in August: the crops did not look much amiss upon the ground, but disappointed people, for they yielded badly, especially beans and pease, of which there were very few; barley and oats were the best; but the scarcity of other things made them also dear. Myrtles flowered very fine this summer, because they began in July, which is sooner than usual; they are apt not to begin till the warm weather is almost over. The harvest was in general well got, but not so well at the end as at the beginning, for there were 18 inches of rain in five months, from July to November; great single rains in July, fits of wet in August and September, and almost daily in October and November, with floods and storms; this made great plenty of grass; but the ground became wet and soft, and much trodden, and the turnips were not so good as might have been expected. The crop of fruit was very uncertain; in some places it was very scarce, in other places there was a good deal; but in most the apples rotted extremely. The hedge fruits were in great abundance, excepting ash-keys, of which most people said there were none at all.

The autumn though wet, was mild; swallows and martins did not go away till about October 18; the autumnal flowers continued till December, anemonies were then in flower; winter and spring flowers were forward, and the leaves of the spring crocus appeared. But the latter half of December the

scene altered, and the frost began; it was a mixture of severe and moderate frost, falling and melting snows, and floods, with hard frost and breaks; the beginning of a very severe winter, which lasted long into the next year.

XX. *An Account of the Trigonometrical Survey carried on in the Years* 1791, 1792, 1793, *and* 1794, *by Order of his Grace the Duke of* Richmond, *late Master General of the Ordnance. By Lieut. Col.* Edward Williams, *and Capt.* William Mudge, *of the Royal Artillery*; *and Mr.* Isaac Dalby. *Communicated by the Duke of* Richmond, *F. R. S.*

Read June 25, 1795.

INTRODUCTION.

A GENERAL survey of the island of Great Britain, at the public expence, was (as we learn from the LXXVth Vol. of the Philosophical Transactions) under the contemplation of Government as early as the year 1763, the execution of which was to have been committed to the late Major General ROY, whose public situation and talents well qualified him for such an undertaking. Various causes procrastinated this event till the year 1783, when the late M. CASSINI DE THURY transmitted a memoir to the French ambassador at London, which paved the way to a beginning of this important work. Calculated for the advancement of science, this memoir was presented to the King, and readily met with the approbation of a monarch, so eminently distinguished, from the æra of his reign, for his liberal patronage of the arts and sciences. By

his Majesty's command, the memoir was put into the hands of Sir JOSEPH BANKS, P. R. S. accompanied with such marks of royal munificence, as speedily obtained all the valuable instruments and apparatus necessary for carrying the design into immediate execution.

General ROY, to whose care the conduct of this important business was committed, lived to go through the several operations pointed out in the memoir, the particulars of which have been detailed at great length in the Philosophical Transactions, where they will remain a testimony of his zeal and ability in conducting so arduous an undertaking at an advanced period of life. The further prosecution of the survey of the island, to which the operations hitherto performed might be deemed only as subservient or introductory, seemed to expire with the General.

The liberal assistance which his Grace the Duke of RICHMOND had on all occasions given to this undertaking; and particularly the essential services performed by Captain FIDDES, and Lieutenant BRYCE, of the corps of royal engineers, in the survey and measurement of the base of verification on Romney Marsh, are acknowledged by General ROY in the strongest terms. A considerable time had elapsed since the General's decease without any apparent intention of renewing the business, when a casual opportunity presented itself to the Duke of RICHMOND of purchasing a very fine instrument, the workmanship of Mr. RAMSDEN, of similar construction to that which was used by General ROY, but with some improvements; as also two new steel chains of one hundred feet each, made by the same incomparable artist. Circumstances thus

concurring to promote the further execution of a design of such great utility, as well as honour, to the nation, his Grace, with his Majesty's approbation, immediately gave directions to prepare all the necessary apparatus for the purpose, which was accordingly provided in the most ample manner.

SECTION FIRST.

An Account of the Measurement of a Base on Hounslow Heath, *with an hundred Feet Steel Chain, in the Summer of the Year* 1791. Reference to be had to Tab. XLIII. and XLIV.

ARTICLE I. *Preamble.*

Previous to entering upon the ensuing account, it may not, perhaps, be improper to enumerate some preliminary matters relative to the subject. The first mode of mensuration adopted by General Roy was that with deal rods, which had also been used and approved of in other countries. In the course of the measurement, however, it appeared, that the sudden and irregular changes which these rods were liable to, from dryness, humidity, or other causes, rendered them totally unfit for ascertaining the length of the base with that degree of precision, of which it was at first thought they were capable. On this account they were laid aside, and glass rods substituted in their stead. These rods were contrived with great ingenuity to answer the purpose, as fully appears by the account given of them in the Philosophical Transactions. But this mode of mensuration being the first of the kind, seemed to require

some proof of its accuracy, which consideration induced General Roy to make a comparison between the glass rods and the steel chain, which Mr. Ramsden had made for the Royal Society. For this purpose a distance of one thousand feet was carefully measured with the rods and the chain. The result of these measurements appeared to be such as would have produced a difference of little more than half an inch upon the whole base, had it been measured with each of them respectively. But notwithstanding the apparent degree of accuracy which this, or any other mode of measuring may be supposed capable of, yet it seems necessary that every base, intended to become the groundwork of such nice operations, ought always (when circumstances will permit) to be measured twice at least.

The manner in which the glass rods were applied in the measurement, is supposed to have rendered the operation liable to some small errors, which lying different ways, might possibly have counterbalanced each other, and produced a true result: but this supposition ought never to be admitted in experimental inquiries, unless such errors can be nearly estimated. The principal cause of error is supposed to arise from the ends of the two adjacent rods being made to rest on the same tressel; because when the first rod is taken off, the face of the first tressel, being then pressed by the end of one rod only, will acquire a tendency to incline a little forward. The error arising from this cause will evidently tend to shorten the apparent base.

Another source of error is supposed to arise from the casual deviation of the rods from a right line, in the direction of the base, tending to increase its apparent length. And a third

error is supposed to result from the method which was used, of supporting the ends of the rods on two tressels only, by which they become liable to bend in the middle. This concave form of the rods would also tend to lengthen the base. The first of these causes of error was submitted to experimental inquiry in the garden of Richmond house, Whitehall, in the presence of his Grace the Duke of RICHMOND, Sir JOSEPH BANKS, Mr. RAMSDEN, and Mr. DALBY; when it appeared evidently, that the glass rod had a small motion when the other rod, which had counterbalanced it, was taken from the tressels.

These considerations, therefore, rendered it necessary to compare the measurement with the glass rods, with that performed by some other method; not on account of any doubt being entertained of the care with which General ROY'S operation had been performed, but solely with a view to bring this new mode of measuring to some proper test. No method of comparison could, perhaps, be better than measuring the same base with the steel chain. General ROY himself, in his remarks on the comparative accuracy of the two bases, that of Hounslow Heath and Romney Marsh, evidently gives the preference to the chain; which, every circumstance considered, it is certainly right to do. These reasons induced his Grace the Duke of RICHMOND to direct the base on Hounslow Heath to be remeasured with the steel chain; and although the result does not differ from the glass rods by so small a quantity as General ROY'S experiment assigned, yet it does not amount to more than three inches on a base exceeding five miles.

ART. II. *Of the Apparatus provided for the Measurement of the Base.*

The apparatus, provided for the measurement, consisted of the following articles, *viz.*

1. A transit instrument.
2. A boning telescope.
3. Two steel chains, 100 feet each, with the apparatus for the drawing-post and weight-post.
4. Fifteen coffers of deal, for receiving the chain when extended in a right line.
5. Thirty-six strong oaken pickets of $3\frac{1}{2}$ and $4\frac{1}{2}$ feet long; shod, and hooped with iron.
6. Four brass register heads, carrying graduated sliders moved by finger-screws, for adjusting the ends of the chain. One of these registers has a micrometer-screw attached to it, proper for measuring small quantities expanded or contracted by the chain.
7. Thirty-six cast iron heads, to fix on the pickets.

As many of these articles have been described very circumstantially by General ROY in the LXXVth and LXXXth Volumes of the Philosophical Transactions, it will only be necessary here to give a description of the transit instrument, boning telescope, and the two new chains.

1. *The Transit Instrument.* Tab. XLIII.

This instrument, made by Mr. RAMSDEN, may be considered as a transit combined with a telescopic level, which

makes it serve two purposes; one for determining points in the same vertical plane; the other to show how much a measured line deviates from the level. It consists of a telescope about eighteen inches long, with an achromatic object-glass of about $1\frac{6}{10}$ inches diameter. The telescope passes through an axis in the manner of a transit, and as it must be used for viewing objects at very different distances, the images from the object-glass will vary in the same proportion; it therefore becomes necessary to vary the distance of the wires, so that they may be exactly in the same place with the image. For this purpose there is a pinion, moveable by turning a milled head at A, whereby the small tube, with the wires which are contained in the box B, are made to approach, or recede from the object-glass.

The two pivots, or extremities of the axis, are made with great accuracy to the same diameter; and they turn in angles in the uprights C and D. Each of the angles is fixed in a slider; one at D, to move horizontally, by turning a finger-screw E; the other vertically, by turning the finger-screw F.

The level G is here represented as suspended by its hooks on the transverse axis. Its use is to shew when that axis is horizontal; and it is furnished with an adjusting-screw H, by which the two hooks may be made exactly of the same length, so that the axis on which it is suspended may become parallel to a tangent to the middle of the glass tube. This level also serves to set the line of collimation in the telescope horizontal; for which purpose there are two pins, K and L, attached to the side of the telescope, parallel to the axis thereof: one of these pins is furnished with an adjusting-screw M, by which the

the line of the hooks is made parallel to the line of collimation in this direction, with the greatest precision. The level may be suspended on these pins in the same manner as on the horizontal axis.

The cross wires at N, in the common focus of the object and eye-glasses, are fixed at right angles to each other; but instead of being placed horizontally and vertically, as in the common way, they make each an angle of 45° with the plane of the horizon. This mode of fixing wires is of the greatest advantage in making nice observations, as it remedies the inconvenience and error arising from their thickness. To bring the line of collimation in the telescope at right angles to the horizontal or transverse axis, there are two nuts for the purpose, one on each side of the box at N, which serve to move the intersection of these wires towards the right or left.

In the eye end of the telescope is a micrometer, which serves to measure small angles of elevation or depression. It consists of a moveable horizontal wire, placed as close as possible to the cross wires already mentioned. By turning the micrometer-screw O, this wire is moved across the field of the telescope, and the space which it moves through is shown in revolutions of the micrometer-screw, by means of an index, moveable in a slit, and the divisions on the stem Q. The parts of a revolution are shown in 100ths by an index P, on the micrometer head.

In tracing out a base by intermediate stations, the instrument must be frequently shifted to the right or left, till the telescope shows that the middle of its axis and the extremities of the base are in the same vertical plane. To expedite this

operation, there are slits cut through the top of the mahogany board, for receiving the screws which fasten the supports of the telescope; by which means the telescope, with its supports, can be moved a little to the right or left, whilst the stand remains fixed. Over another slit in the top, and directly under the centre of the axis of the telescope at R, is a small hole for a wire or thread to pass through, suspending a plummet for marking a point on the ground, when the telescope is brought into the desired vertical plane.

The method of levelling the axis, adjusting the line of collimation, &c. are similar to those for the upper telescope of the great theodolite, as described in the Philosophical Transactions.

2. *The Boning Telescope.*

This telescope is in every respect the same as that which was made use of by General Roy, therefore it will only be necessary to explain the application of it, for fixing the pickets in the direction of the base, with the tops of those belonging to the same hypotenuse in the same right line.

A rope being stretched along the ground, in the direction of the base, distances of 100 feet were marked upon it by means of a twenty-feet deal rod. After a sufficient number of these distances were set off, the telescope was laid on a narrow piece of board, truly planed, and fixed to the top of the picket at the beginning of the hypotenuse; and another picket was driven into the ground at a convenient height at the other end. To the top of this last, a thin deal spar was fixed, and the telescope directed to it, whilst the intermediate pickets were driven to

their proper height. To determine this height more accurately, another spar, whose thickness was equal to the height of the axis of the telescope above the top of the picket, which supported it, was repeatedly laid on the top of each picket at the time of driving it, till its upper edge and the fixed spar appeared in a right line. Whilst the pickets were driving, they were moved a little to the right or left, as directed by signals from the observer at the telescope, till their tops appeared in the same right line.

3. *The Chains.*

These chains were made by Mr. RAMSDEN, and are of similar construction in the joints to that which he made for the Royal Society, described in the LXXVth Volume of the Philosophical Transactions; but they differ from that in other respects. Instead of one hundred links, each of these new chains contains forty, of $2\frac{1}{2}$ feet long. The link is in form of a parallelopipedon, of half an inch square, which renders it considerably stronger than that of the Royal Society; and the chain having fewer links, becomes less liable to apply itself to any irregularities which the coffers may be subject to. The handles are of brass, and being perfectly flat on the under side, they move freely upon the brass register-heads, by which means the coincidence between the arrows at the extremities of the chain, and the divisions on the scales, are readily and accurately obtained. The two chains will hereafter be distinguished by the letters A and B.

On Saturday July the 23d, all the foregoing articles were conveyed from the Tower to the end of the base near King's

Arbour, where tents were pitched for a party of the royal regiment of artillery, consisting of one serjeant and ten gunners, who were to be employed in the laborious part of the operation.

ART. III. *Experiments made to ascertain the relative Lengths of the Chains, before and after they were used; and also to determine the Expansion of one Chain, or one hundred Feet of blistered Steel, by one Degree of* FAHRENHEIT'*s Thermometer.*

For this purpose, two strong oaken pickets were driven two feet into very firm ground, and the drawing-post was made fast to them. Five coffers were arranged in a right line, and supported upon courses of bricks. The chain was then placed in the coffers, and stretched with a weight of fifty-six pounds. Notwithstanding the great resistance which it was thought these pickets were capable of, yet it was found insufficient to counteract the friction between the coffers and the chain, when the expansion or contraction took place. Three pickets, therefore, of forty-four inches long, were driven into the ground, within six inches of their tops, and the drawing-post was fastened to them by several folds of strong rope. The pickets and rope were also covered with earth, to prevent their being warped by the sun.

The micrometer-screw, attached to the brass register-head, by means of which the expansion or contraction was measured, contains 26 threads in an inch. The circular head is divided into 10 equal parts, and consequently each division will measure $\frac{1}{260}$th part of an inch. But as the eye readily subdivides

each of the divisions into 4 parts, the micrometer will measure the $\frac{1}{1040}$th of an inch tolerably exact.

For finding the relative Lengths of the Chains.

In order to accomplish these experiments in the most unexceptionable manner, after the chain was properly stretched in the coffers, and the thermometers placed by it, the whole remained till all the thermometers stood steadily at the same height. The ends of the chain being then in perfect coincidence with particular divisions on the brass register-heads, the chain was quickly taken out and replaced by the other, which being properly stretched in a right line, and a coincidence made at the drawing-post end of the chain, the variation of the other end from the division on its register-head showed the difference of the lengths of the chains, which was measured by the micrometer. As it required weather particularly steady to succeed in these experiments, we were obliged to catch the most favourable opportunities that presented themselves, which happened on the 29th and 30th of July; on those days the chains were compared with each other, and the results were as follow.

July 29th. *Thermometers remaining steadily at 75° during and after the operation.*

The chain B was found to be $6\frac{1}{2}$ divisions of the micrometer head shorter than the chain A; and on being shifted, A was found to exceed B $6\frac{1}{2}$ divisions.

Same day. Thermometers steady at $67\frac{1}{2}$°.

The chain B 6 divisions shorter than A; and being shifted, the chain A was 6 divisions longer than B. The mean from these experiments is, A $6\frac{1}{4}$ divisions longer than B.

In the table containing the particulars of the operation it will be found, that the chain B was laid aside after measuring 38 chains, on account of one of the links appearing to be a little bent. Before it was sent to Mr. Ramsden it was compared with the chain A (at first intended to be kept as the standard chain), when it was found to be only $4\frac{1}{2}$ divisions longer; which being $1\frac{3}{4}$ divisions less than the mean $6\frac{1}{4}$ as found above, shows, that the chain B had lengthened $1\frac{3}{4}$ divisions in measuring 38 chains; for when Mr. Ramsden afterwards straightened the link, he could not perceive any difference in its length.

The remainder of the base was measured with the chain A (the chain B being kept as a standard), and when that was completed, a comparison was again made between A and B, when it appeared that A exceeded B by $14\frac{2}{10}$ divisions of the micrometer head; therefore the wear of A, by lengthening of the joints, in measuring 236 chains, was $14,2 - 4,5$ divisions $= 9,7$ divisions of the micrometer.

For finding the Rate of Expansion.

The chain being placed in a right line, along the horizontal bottoms of the coffers, and kept in a state of tension by a weight of fifty-six pounds, five thermometers were placed close by the chain; one in the middle of each coffer; and the whole was covered with a white linen cloth, when the sun shone out. After remaining a few minutes, till the thermometers were nearly of the same temperature, a perfect coincidence was made on the register heads, at each end of the chain, and the thermometers noted. Every thing remained in this state till the coincidence at the weight end of the chain was ob-

served to be altered, and the thermometers nearly the same; at which instant, they were again read off, and the alteration of coincidence measured by the micrometer.

August 5th, cloudy.

Thermometers.					Mean.	Micr. Divisi^s.	Total contr. Inches.	Contr. on 1°. Inches.
1	2	3	4	5				
75,75 62,5	75,5 62,75	76 63	76,25 63	76 63	75,9 62,85	$25\frac{1}{8}$	,096642	,0074

Here the contraction of the chain is $25\frac{1}{8}$ divisions of the micrometer $= 25\frac{1}{8} \times \frac{1}{260}$ inches $=$,096642 inches, and the corresponding variation of the thermometers, taking the difference of the means, is 13°,05; consequently the contraction on $1° = \frac{,096642}{13,05} =$,0074 inches.

Aug. 6th, cloudy.

89,5 69,5	89,75 69,5	90 69,25	90 69	90,5 69,5	89,95 69,35	38,5	,148077	,00719

Aug. 7. Coffers covered with the linen cloth.

102,5 87 89 98 93	102,5 86 89,75 95 92	102,75 87 93 102 96	102 88 92 99,75 95	102 88 92 101 95,75	102,85 87,2 91,15 99,15 94,35	29,5 8 16,25 9,33	,113462 ,030769 ,062500 ,035885	,00749 ,00779 ,00781 ,00748

Aug. 7th, in the evening. Coffers covered with the linen cloth.

Thermometers.							Total contr.	Contr. on 1°.
1	2	3	4	5	Mean.	Micr. Divisi.	Inches.	Inches.
90	91	89	91	92	90,6			
80	80	81,5	81,5	81	80,8	19	,073077	,00746
67	68	69,5	69	69	68,5	23,5	,090385	,00735
60,75	62,75	62	62	62	61,8	13	,050000	,00746

The mean result from these nine experiments is 0,007492, or 0,0075 inch to 1° of FAHRENHEIT, on 100 feet of blistered steel; which differs only $\frac{13}{100000}$th parts of an inch from General ROY's conclusion with the pyrometer; but the number ,0075 is preferred in these measurements, as being deduced from experiments made with the chain itself.

ART. IV. *Particulars relative to the Commencement of the Operation, &c.*

After the chains were compared, and the rate of expansion determined, as related in the preceding article, several trials were made of arranging the pickets and coffers in such a manner as might be supposed proper for the reception of the chain. It was soon found, however, that this method of measuring would be neither so expeditious or accurate, as if the coffers were placed upon tressels, such as were made use of by General ROY in his measurement with the glass rods. An application was therefore made to Sir JOSEPH BANKS, who very

obligingly complied with the request, and lent the tressels belonging to the Royal Society; a description of which may be seen in the LXXVth Vol. of the Philosophical Transactions.

As the upper part of the pipe at the north-west end of the base was found to be exceedingly rotten, it became necessary to saw off 13 inches of it, which left enough of the cylinder remaining to fix the brass cup in, as it had been originally bored to the depth of two feet. This cup, which was also lent by the Royal Society, being inserted in the pipe, fitted it exactly.

On the 15th of August, having previously traced out the line of the base, by means of the transit instrument, the operation commenced, in the presence of Sir JOSEPH BANKS, Dr. MASKELYNE, and several other members of the Royal Society. The following table, which contains the particulars of it will explain the order of time in which the different parts of the measurement were performed. As it would swell this table to a great extent, were the degrees shewn by the thermometers inserted therein, it has been considered as proper to give only their sum, which is sufficient for finding the correction to be applied in the reduction of the base, on account of the lengthening or contracting of the chain by variation of temperature. It may, however, be remarked, that the five thermometers were laid close by the chain, and suffered to remain till they had nearly the same temperature, when they were read off, and registered in a field-book, whilst an observer at each end of the chain preserved a perfect coincidence between the arrow and a particular division on the brass scale. When the sun shone out, the chain was covered with a white linen cloth, the ends of which were put over the openings of

the first and last coffers, to exclude the circulation of air. The thermometers usually remained in the coffers from 7 to 15 minutes, according to circumstances; when the sky was much overcast, a shorter time generally was found to be sufficient.

ART. V. Table, containing the Particulars of the Measurement: the first Column showing the Day of the Month when each Hypotenuse was finished; the Second, the Number of Hypotenuses; the Third, the Number of Chains in each Hypotenuse; the Fourth, the Perpendicular belonging to each Hypotenuse, or the *datum* for reducing it to the Plane of the Horizon; the Fifth, the computed Reduction; the Sixth, the new Points of Commencement above or below the Head of the last Picket when a new Direction was taken; the Seventh, the total Descent of the Extremity of each Hypotenuse; and the Eighth, Remarks, or general Occurrences.

1791. Month.	No. of hypoten.	No. of chs. in hypoten.	Perpendicular.	Reduction of hypotenuse.	New point of commt.	Total descent.	Remarks.
			Inches.	Inches.	Inches.	Inches.	The 1st chain commenced 14 inches above the head of General Roy's pipe before it was cut off smooth.
Aug. 15	1	3	5,8	0,00467	1,8	19,8	
16	2	3	0,0	0,00000		21,6	
22	3	32	57,	0,04231		35,4	
23	4	14	26,25	0,02051	4,9	61,65	Began measuring with chain A at 4th hyp. one of the links on the chain B appearing to be a little bent.
25	5	10	12,1	0,00610	7,9	68,85	
29	6	19	0,0	0,00000		60,95	
Sept. 2	7	34	28,8	0,01017		89,75	[8th hypot.
4	8	1	3,8	0,00602		85,95	Crossed the river Coln at the
6	9	15	69,25	0,13321	4,25	155,20	Crossed the Staines road at the 9th hypotenuse.
8	10	17	15,3	0,00574		166,25	
9	11	5	33,5	0,09352		199,75	
12	12	13	1,9	0,00012	8,25	201,65	
12	13	7	54,5	0,17680		247,90	
13	14	6	0,0	0,00000	5,25	247,90	
14	15	5	7,5	0,00469		250,15	
14	16	9	0,0	0,00000	9,5	250,15	
16	17	8	5,3	0,00146		245,95	
17	18	10	2,9	0,00035		248,85	
20	19	5	4,8	0,00192		253,65	
20	20	4	8,1	0,00683		261,75	
21	21	8	1,5	0,00012		260,25	
21	22	6	35,4	0,08703		295,65	
22	23	1	6,4	0,01707		302,05	Crossed the Wolsey river at the 23d hypotenuse.
23	24	10	14,5	0,00876		316,55	
25	25	12	54.4	0,10275		370,95	
25	26	1	24,5	0,25015		346,45	
25	27	5	1,0	0,00001		345,45	
26	28	5	11,3	0,01064		356,75	
26	29	1	9,0	0,03375		365,75	The head of the last picket was 2½ feet above the head of the pipe before it was cut off smooth.
26	30	5	6,9	0,00397		372,65	
	Total reduction =			1,02867	=0,08572 feet.		

Sum of all the degrees shown by the thermometers = 96795,25.

ART. VI. *Further Remarks; and Reduction of the Base to the Temperature of 62°.*

Remarks.

It having been our wish, that some scientific persons should be present at the completion of the measurement, his Grace the Duke of RICHMOND was pleased to desire Dr. MASKELYNE, astronomer royal, and Dr. HUTTON, professor of mathematics in the royal military academy at Woolwich, to attend upon this occasion; to whom Mr. RAMSDEN was necessarily joined, as his standard brass scale, and beam compasses, were requisite to conclude the business with the wished for accuracy. Accordingly, on Wednesday the 28th of September the remaining three chains were measured in their presence; and the horizontal distance from the end of the last chain to the axis of the pipe was found to be 21,055 inches, as determined by Mr. RAMSDEN; and consequently the apparent length of the base was 274 chains, and 21,055 inches.

The height of the last picket above the pipe was 35 inches, from which deducting the 5 inches of the rotten part, which was cut off, there remains 30 inches, or $2\frac{1}{2}$ feet, for the height of the last picket, above General ROY's pipe; which makes the whole descent 33,55 feet; or about $2\frac{1}{4}$ feet more than was determined by the former measurement.

Reduction of the Base to the Temperature of 62°.

	Feet.
Apparent length, namely, 274 chains + 1,755 feet - - - - - -	27401,755
The correction for the excess of the chains lengths* above 100 feet, and half their wear, is $\frac{236 \times ,0956 + 38 \times ,05489}{12}$; and this add - -	2,0539
The sum of all the degrees shewn by the thermometers was 96795,25; therefore $\overline{\frac{96795,25}{5} - 54°} \times 274 \times \frac{,0075}{12}$ is the correction for the mean heat in which the base was measured, above 54°, the temperature in which the chains were laid off; and this also add - - - -	2,8519
Hence these corrections, added to the apparent length, give - - -	27406,6608
Again, for the reduction of the base to the temperature of 62° we have $\frac{8°}{12} \times 3,38938$; and this subtract - - - - - -	2,2596
By the table, the sum of all the corrections for reducing the several hypotenuses to the plane of the horizon is 1,02867 inches = 0,08572 feet; and this subtract - - - - -	0,0857
Hence these corrections taken from the above length leaves that of the base in the temperature of 62° - - - - -	27404,3155

* For the lengths of the chains A and B see ART. VII. of this section.

To compare this length of the base with that assigned by General Roy, it becomes necessary to rectify a small oversight in the 4th step of the process published in the Philsophical Transactions for 1785.

The equation for 6° difference of temperature there specified, should consist of the *difference* of the numbers for brass and glass, and not of that for brass alone, *viz.* $\frac{6^\circ}{12} \times \overline{3,38938 - 1,41658} = 0,9864$ feet instead of 1,6946, which makes the base 0,7082 feet too long. Therefore the length of the base, as measured by the glass rods, is 27404,0843 feet, being about $2\frac{1}{4}$ inches less than by the above reduction; consequently 27404,2, the mean of the two results, may be taken as the true length of the base.

ART. VII. *Mr.* Ramsden's *Method of ascertaining the actual Lengths of the Chains* A *and* B. Tab. XLIV.

These chains were originally compared with the brass points inserted in the stone coping of the wall of St. James's churchyard; but the temperature at the time of that comparison was afterwards forgotten by Mr. Ramsden. After the mensuration on Hounslow Heath was finished, the chains were again compared with those points; but the result did not prove to be satisfactory, as there were reasons for supposing that some alteration had taken place in the length of the coping; but, independent of this, the great irregularities between the joints of the stones, some of which projected half an inch above others, rendered it at best a very rude and inaccurate operation. Mr. Ramsden had points remaining on his great plank, which had been transferred from the brass standard, but as the plank

itself was found to be subject to a daily expansion and contraction, he turned his thoughts to the invention of some other method of measuring the lengths of the chains, in a more unexceptionable manner.

On considering that the expansion of cast iron is nearly the same as that of the steel chain, he procured a prismatic bar of that metal, of 21 feet long, judging it to be the most proper material for the present occasion, as well as for establishing a permanent standard for future comparisons of the same kind. The manner in which the bar was fitted up for the purpose will be readily understood by attending to Tab. XLIV.

The great plank was cut to the length of about 22 feet, and on one of its narrow edges 21 brackets were fixed; each of which had a triangular notch to receive and support the bar, with one of its angles downwards, so that the upper surface became one of the faces of the prism. Before the brass points were inserted in this bar, Mr. RAMSDEN compared his brass standard with that belonging to the Royal Society, for which purpose, on Nov. 22d, 1791, it was sent to their apartments in Somerset house, where, after the two standards had remained together about 24 hours, they were found to be precisely of the same length. Brass points were then inserted in the upper surface of the bar, from Mr. RAMSDEN's standard, at the distance of forty inches from each other, the whole length of 20 feet being laid off on those points in the temperature of 54°.

The chains were measured in the Duke of MARLBOROUGH's riding-house, where the light was very convenient for the purpose, and the whole apparatus was sheltered from the wind and sun. The plank and bar were supported on five of the tressels, or tripods, belonging to the Royal Society, and the upper sur-

face of the bar was brought into an horizontal plane by means of screws and a spirit level. The brass points on the upper surface of the bar were brought into a right line, by stretching a silver wire along the top, and pressing the bar laterally with wedges, till all the points fell under the wire. Part of the chain was then placed on rollers, which rested on narrow slips of wood fixed on the side of the plank, about five inches below, and exactly parallel to the bar; and whilst it was fastened to an adjusting-screw near one end of the plank, it was kept straight on the rollers by a weight of fifty-six pounds.

From the extremities of the 20 feet on the edge of the bar, two fine wires with plummets were suspended, which were immersed in vessels of water, the wires hanging so as nearly to touch the chain. One end of the chain being then brought under its wire, by means of the adjusting-screw, a fine point was made on the chain coinciding with the other wire. This part of the chain was then shifted, and another 20 feet measured in the same manner; and the operation continued till the length of each chain was thus obtained at five successive measurements. The result was, that in the temperature of $51\frac{1}{2}°$, in which the operation was performed, the chain A was found to exceed 100 feet by 0,114 inches, and the chain B, by 0,058 inches. Now, according to the table of expansions in Vol. LXXV. Phil. Trans. the expansion due to 1° FAHRENHEIT on 100 feet of cast iron is 0,0074 inches, and that of the chain being 0,0075, their difference is 0,0001, and therefore for $2\frac{1}{2}°$ it will be 0,00025; consequently, as the points were put on the bar in the temperature of 54°, and the chains measured in $51\frac{1}{2}°$ or $2\frac{1}{2}°$ less, their lengths in the temperature of 54°, agreeing with the points on the bar, will be

	feet.		inches.
A =	100	+	0,11425
B =	100	+	0,05825

The comparison of the chains with each other, as related in ART. III. together with this determination of their lengths, furnish the *data* necessary for the reduction of the base on Hounslow Heath.

The wear of B, in measuring 38 chains, appeared (vid. ART. III.) to be $1\frac{3}{4}$ divisions of the micrometer head $= \frac{1,75}{260} =$ 0,00673 inches: and the wear of A was 9,7 divisions $= \frac{9,7}{260} =$ 0,0373 inches.

Then, from the excess of A above	Inches.		Inches.
100 feet, namely, - -	0,11425,	and of B	0,05825
subtract half the wear -	0,01865		0,00336
	6,0956		0,05489

And we get the lengths of the chains in the temperature of 54 deg. before they were used in the measurement, namely,	A = 100 Ft. + ,0956 In., and B = 100 + ,05489, the lengths used in the reduction of the base.

ART. VIII. *Method of fixing the Iron Cannon at the Extremities of the Base on* Hounslow Heath, 1791.

As the pipes were found in a very decayed state, and it became certain, were they suffered to remain as the *termini*, that in a few years the points marking the extremities of the base would be lost, it became necessary to re-establish them in a more permanent manner. Amongst the various means

which were proposed for this purpose, that of heavy iron cannon was adopted, having been previously sanctioned with the approbation of Mr. RAMSDEN, and other competent judges. Two guns were therefore selected at Woolwich by order of the Master-general, from among those which had been condemned as unfit for the public service, and sent to Hampton by water.

The placing of these guns accurately being an operation of a delicate nature, and attended with some difficulty, on account of their great weight, the mode of performing it was very deliberately considered; and every precaution afterwards taken to render the operation unexceptionable. The method was as follows.

Four oaken circular pickets, of 3 inches diameter, were driven into the ground, at the distance of 10 feet each from the centre of the pipe, two of them being in the direction of the base, and the others at right angles to it. Melted lead was then run into a hollow made in the head of each picket, and afterwards filed off perfectly smooth. On the brass cup, belonging to the Royal Society, being adjusted in the pipe, silver wires were stretched from the heads of the opposite pickets, and moved till their intersection coincided with the centre of the cup; and in this position a fine line was drawn on the lead of each picket, exactly under and in the direction of the wire. This operation being performed, and the truth of it reexamined, the pipes were taken out of the ground, in doing which it became necessary to make an excavation of about four feet, in order to clear the circumference of the wheel. It had been at first intended to have inserted the gun so far in the ground as that its muzzle should be even with the surface of

the original pipe: but upon considering that this was a matter not absolutely essential to the ascertaining of the actual length of the base by any future measurement, provided the axes of the guns were made to coincide with those of the pipes, it was determined to fix the cannon, without digging the pit to a greater depth than that of ten feet. In this position, however, it was evident, that the muzzle of the gun would rise higher than the surface of the pickets, which had been put into the ground for finding the centre; which rendered it necessary to drive in and adjust four outer pickets, of a proper height, to determine the centre of the bore of the gun, by the intersection of another set of wires. The tops of the first set of pickets were therefore cleared, and the silver wires extended along the fine lines which had been made on the lead. A plummet was then suspended from above, and moved till it fell on the intersection of the wires. Being fixed in this position, another set of wires was stretched across the tops of the four outer pickets, till their intersection also coincided with the vertical wire of the plummet, in which position, fine lines were drawn under the wires on the top of each of the outer pickets. The truth of the operation now depending on these last pickets, they were carefully guarded by another set which surrounded each of them, and these last were again bound round with ropes, to preserve the centre pickets from any possible accident. These precautions being taken, and the pit cleared, a large stone of $2\frac{1}{2}$ feet square, and 15 inches deep, containing a circular cavity in its upper surface to receive the cascabel of the gun, was placed in the bottom of it, the centre of the hole being nearly under the intersection of the wires, as determined by a plummet. The gun was then

let into the pit, and resting upon the stone, it was brought into a position nearly vertical, at which time a quantity of earth and stones were thrown into the pit sufficient to steady the gun. This being done, the cross wires were stretched over the outer pickets, and a pointed plummet suspended from above, having its line coinciding with the intersection of the wires, was let fall into the cylinder, in which a cross of wood that exactly fitted it was placed, whose centre corresponded with that of the bore. The gun was then moved till a dot marking the centre of the cross came directly under the point of the plummet; when earth and stones were rammed round the gun, care being taken to force it by that operation into its proper position, as shown by the plummet and cross. In this manner were the guns fixed at the extremities of the base; and it remains only to be observed, that to prevent the unequal settling of the earth, rammed within the pit, from moving them out of their proper positions, four beams of wood were placed in an horizontal direction, having their ends resting against the sides of the pit and the gun. It may also be added, that iron caps were screwed over the muzzles to preserve the cylinders from rain.

SECTION SECOND.

Containing Particulars relative to the Commencement of the Trigonometrical Operation.—An Account of the Improvements in the great Theodolite; and a Relation of the Progress made in the Survey in 1792, 1793, and 1794, together with the Angles taken in those Years.

ART. I. *Of Particulars relative to the Commencement of the Trigonometrical Operation.*

Having, by the re-measurement of the base on Hounslow Heath, sufficiently determined its accuracy, it became necessary, upon the approach of the following spring, to form some plan which might enable us to commence the survey with the most advantage.

Of those which were suggested, that of proceeding immediately to the southward with a series of triangles seemed the most eligible, not only because, in the first instance, the execution of it would forward one great design of the business, in an early determination of some principal points upon the sea-coast, but also because a junction of the eastern part of the series with that of the western of General ROY, would afford an early proof of what degree of accuracy had attended both operations.

To ascertain the truth of the General's work, by verifying some principal distance or distances, was an object which presented itself, not only as interesting and curious, but as highly necessary, in order to determine whether, by the result, the triangles might stand good, and become a part of the general series.

In addition to this reason, there was another which offered itself, and that was, the prospect of being able to obtain the length of a degree of longitude in an early stage of the survey; for it had been suggested, and upon inquiry was found to be true, that Dunnose in the Isle of Wight was visible, in particular moments of fine weather, from Beachy Head on the coast of Sussex: but attention was at the same time given to the recommendation of General Roy, in the selection of Shooter's Hill and Nettlebed, as places for observing the directions of the meridian; and it was resolved, whatever preference might in future be given to those on the coast for this important operation, that at all events such observations should be made, as might determine the distance between the stations recommended by the General.

Having therefore formed an outline for the operation of the year 1792, upon the approach of spring, Captain Mudge and Mr. Dalby explored the country over which it was intended to carry the triangles, and visited such stations in the series of General Roy as were judged to be proper for the above purpose.

In the choice of those stations which were about to be selected, instructions had been given by his Grace the Duke of Richmond to avoid towers and high buildings, as getting an instrument on them had, by the experience which the former operation afforded, been found difficult and dangerous; such of them therefore as were thus circumstanced were avoided, and near the most proper ones, stations were chosen on the ground. From these directions the points of junction were necessarily confined to Saint Ann's Hill, Botley Hill, and Fairlight Down, because the pipe sunk near Hundred Acre House

was found to be destroyed; but this was considered immaterial in its consequence, as it would have been improper to have chosen it for a principal station, because the high ground near Warren Farm took off the view of Leith Hill.

A disadvantage however, which seemed to result from this resolution of avoiding high buildings for stations, occurred in the difficulty which offered itself of proceeding from Hanger Hill and St. Ann's Hill, with a mean distance of that side as given by General Roy; for the station chosen at the former place being on the ground, there was scarcely a possibility of erecting a staff at King's Arbour, sufficiently high to afford a view of its top from Hanger Hill: a quadrilateral therefore, similarly posited, could not be fixed on; but as a proper substitute, a station was chosen upon the elevated ground near Banstead, which was visible from St. Ann's Hill, King's Arbour, and Hanger Hill; and this, together with St. Ann's Hill and Hanger Hill, formed two triangles, which would give the distance between St. Ann's Hill and Banstead, independent of each other.

Upon the return of Captain Mudge and Mr. Dalby from their expedition, in which they had selected many of the principal stations, and, by examining the face of the country, had formed some judgment of the future disposition of the triangles, preparations were made for taking the field; and the party which had been engaged in the measurement of the base, were ordered to be attached to the trigonometrical operation.

Little difficulty was found in determining upon the choice of the necessary apparatus. Lamps were constructed, by Mr. Howard of Old-street, which were afterwards found to equal

every thing which could be expected from them. Instead of the reflector being exposed to the wind, these lamps were inclosed in strong tin cases, having plates of ground glass in their fronts, which effectually prevented the bad effects of an unequal and unsteady light. In the centre of the back of each case, there were straps and semicylinders of tin, which moving upon joints, clasped the staff to which in their use they were braced. Two of the lamps were of twelve inches diameter, and a third of twenty-two; and the last of these, prior to the use of it in the ensuing season, was lighted on Shooter's Hill, and clearly distinguished at the distance of thirty miles. Copper nozles of different sizes were likewise provided for holding the white lights.

During the measurement of the base, an observatory for the reception of the instrument was making at the Tower, as likewise two carriages, to be used in conveying them from station to station. One was made with springs for the greater safety of the instrument, which resting upon a cushion in the carriage, was sufficiently secured from any jolting upon the road.

As it was easily foreseen that upon eminences, on which it was certain the instrument would be placed, it would be hazardous to trust it in a receptacle of slight construction, great pains had been taken to make the observatory strong. It consisted of two parts, the interior one of which, or the observatory itself, was eight feet in diameter, and its floor of a circular form, and from the sides of it eight iron pillars rose to the height of seven feet, which were connected at the extremities by oaken braces. The roof was formed of eight rafters which united at the top, having their ends fastened into the heads of

the iron staunchcons, and were otherwise sufficiently clamped. The sides and roof were each composed of four-and-twenty frames, covered with painted canvas, any of which could be removed at pleasure; and the whole was covered with a tent formed of strong materials.

Having thus detailed, in as short a manner as possible, the heads of such particulars as it may be necessary the public should be acquainted with, it remains only to give some account of the improvements in our great theodolite, before we narrate the progress made in the survey in the summer of the year 1792.

ART. II. *Account of the Improvements in the great Theodolite.*

Mr. RAMSDEN has considerably improved this instrument, which, in other respects, is of the same dimensions and construction as that made use of by General ROY, which has already been described in the Philosophical Transactions. The construction of the microscopes render them very superior to those of that instrument; as the means by which the image is proportioned to the required number of revolutions of the micrometer-screw, and also the mode of adjusting the wires to that image, are much facilitated. (See Phil. Trans. Vol. LXXX. p. 146.). For the first, there are three prongs proceeding from the cell which holds the object-glass; these, after passing through slits in the small tube which constitutes the lower part of the microscope, are confined between two nuts which turn on this small tube, so that by turning the nuts, the object-lens is moved towards, or from, the divisions on the circle, as occasion may require. To adjust the wires in the micrometer to the image; in the upper part of the body of the microscope

are two nuts, one sliding within the other. To the upper end of the interior one the micrometer is fixed; and near the lower end are three prongs similar to those above mentioned, but something longer. These prongs pass through slits in the exterior tube, and are confined between nuts, in the same manner as the object-lens. This construction has many advantages over that described in the Philosophical Transactions.

To obviate the necessity of the gold tongue (Phil. Trans. Vol. LXXX. p. 147), besides the moveable wire in the field of the microscope, there is a second one, which may be considered as fixed, having only a small motion for its adjustment. When the instrument is adjusted, and the index belonging to the micrometer-screw stands at the *zero* on its circle (the moveable wire cutting one of the dots on the limb of the instrument), this fixed wire must be made to bisect the next dot; as by this means it may be perceived at any time, whether the relative position of the wire has varied.

By graduating the limb of the instrument to every ten minutes instead of fifteen, we are enabled to measure by the micrometer-screw, not only the excess of the measured angle above any of the ten minutes, but also its complement to the next division on the circle, and thereby to correct any small inequality which may happen between the divisions.

ART. III. *Particulars relating to the Operations of the Year* 1792.

Although it might have been reasonably supposed, that the angles of the triangle King's Arbour, Hampton Poor House, and St. Ann's Hill, had been observed with sufficient accuracy in 1787, yet that this operation might not rest on *data* afforded

by any former one, it was considered as proper to determine them with our own instrument.

By a reference to the Philosophical Transactions, (Vol. LXXX. p. 162.) it will be found, that General Roy was obliged to elevate the instrument at the extremities of the base; for which purpose a stage of thirty-two feet high had been constructed. The same necessity existing with us, an application was made to the Royal Society for it; and in the autumn of 1791, that part of it which had been left at Dover, was brought to the Tower.

The first station to which the instrument was taken this year was Hanger Hill, because it was found upon examination, that the part of the stage which had been left at Shepperton was much damaged, and stood in need of considerable repair. It was, however, soon fitted for use, and a new tent for the top having been provided, the half stage was erected over the pipe at St. Ann's Hill, to which from Hanger Hill the instrument was conveyed. Here, as well as at the other stations where the stage was used, a plumb-line was let fall from the axis of the instrument over the point marking the station, being sheltered from the wind by a wooden trough. In the use of the half stage, the instrument was sufficiently steady when the wind blew moderately; but from the crazy state of the lower part, it was only by watching for moments particularly calm, that satisfactory observations could be made when the whole of it was used.

The following obervations will sufficiently explain the detail of this year's operations, which are given in the order of time in which they were made. By an examination of them it will be perceived, that most of the angles have been observed

more than once: indeed it was a position which we laid down upon our commencing this business, and which, as far as circumstances would admit, has since been adhered to, namely, that of observing the angles upon different arcs. When staffs were erected, which was generally the case when the stations were not more remote than fifteen miles, the angles were repeated till their truth became certain, and the same was also done, when angles were determined by the lamps; but it sometimes happened, that only one of the two white lights, which were burned at the distant stations, was seen; in which case, if the observation appeared to be made without any error, but that which an inequality in the division of the instrument might be supposed to produce, it was considered as sufficient; otherwise fresh lights were sent to the station and observed.

In the use of the white lights, it is conceived that sufficient precautions were taken, as the firing of them was always committed to particular soldiers of the party, selected from the rest on account of their capacity and steadiness, who had instructions to place the copper nozle immediately over the point marking the station, by means of a plumb-line let fall from the bottom. In observing them with the instrument, the angle was not taken till the light was going out. But the men commonly guarded against the flame being blown greatly on one side, by erecting something to windward of the light.

In the use of the lamps also, care was taken to give them their proper direction; for when the ground about the station would not admit of the lamp being placed immediately upon it, slender staffs were erected supported by braces, and made upright, by being plumbed in directions at right angles to each other. Precautions were also used to put those staffs pre-

cisely over the points, by centering the holes in the cross-boards.

To such a part of the staff as was judged to be the most convenient, the lamp was buckled, and its direction obtained by bringing a mark in the middle of it to correspond with another on the staff, which was determined to be opposite the station, by directing a ruler to it from each side of the staff, and marking the places touched. The distance between those marks was then bisected, which gave the situation for the middle of the lamp.

In a very early stage of the business it was found, that the effects of heat and cold on the limb of the instrument were likely to produce the greatest errors; for if the canvas partitions, forming the sides of the observatory, were open to windward, streams of air passing unequally over the surface of it would cause such sudden effects, that little dependance could be placed on any observations made with the instrument in such a state. To avoid this; it was the constant practice when the wind blew with any degree of violence, to prevent the admission of it as much as possible, by keeping up the walls of the external tent, leaving only a sufficient opening for the discovery of the lamp or light; and at other times when the wind blew moderately, and a greater difference appeared in the readings of the opposite microscopes, than an error in division might be supposed to produce, the walls of the external tent were entirely thrown down, and the instrument kept in an equal temperature by the admission of air on all sides.

In taking the angles, it was a general rule for some person to keep his eye at one of the microscopes, and bisect the dot, as the observer moved the limb with the finger-screw of the

clamp. This precaution is very necessary when white lights are used, for should there be a mistake in reading off an angle, when several are taken from the same lamp as the permanent object, it sometimes may prove troublesome to rectify the error, without sending other white lights to the stations. We found that to be the case at Ditchling Beacon, when only one person happened to be at the instrument, and a reading was set down 10″ wrong. A similar circumstance occurred at Brightling. For these reasons, lamps are greatly preferable to white lights, when the distances are not too great.

As the instrument was sometimes found to sink on the axis, which was partly owing to wear by the constant use of it, and the screws of the centre work loosening a little by the shaking of the carriage; whenever it came to a new station, the opposite points were examined; and if it was found that the circle had fallen, which would be shown by the runs of the micrometers, it was raised a little, and the microscopes re-adjusted.

At the different stations, after the observations had been made, large stones, from a foot and a half to two feet square, were sunk in the ground, generally two feet under the surface, having a hole of an inch square made in each of them, whose centre was the precise point of the station.

ART IV. *Angles taken in the Year* 1792.

At Hanger Hill.

Between	°	′	″	Mean. ″
Shooter's Hill and Banstead	62	18	49,5	
			49,75	50,25
			51,5	

Between	°	′	″	Mean. ″
St. Ann's Hill and Banstead -	62	40	34,75 34,75 35,75	35
St. Ann's Hill and Hampton Poor House	24	39	16,5 16,5 17,75	17

At St. Ann's Hill.

	°	′	″	Mean. ″
King's Arbour and Hampton Poor House	44	18	51,5 52 53,25	52,25
Hind Head and Banstead -	90	43	33	
Banstead and Hanger Hill -	63	56	46,5 47 47,5	47
Leith Hill and Banstead -	44	3	3	
Leith Hill and Hind Head -	46	40	30,5	
Bagshot Heath and Banstead	144	39	26	
Hanger Hill and Hampton Poor House	25	17	40,5 41	40,75
Banstead and Hampton Poor House	38	39	6 6,25	6
Shooter's Hill and Hanger Hill	30	28	17 17	17

At King's Arbour.

	°	′	″	Mean. ″
St. Ann's Hill and Hampton Poor House	74	14	35 35,75	35,25
St. Ann's Hill and Banstead -	71	46	23 23,5	23,25

At Hampton Poor House.

	°	′	″	Mean. ″
St. Ann's Hill and King's Arbour	61	26	33,5 35,5	34,5

Between	°	′	″	Mean. ″
St. Ann's Hill and Hanger Hill -	130	3	3 3,5	3,25
At Banstead.				
Shooter's Hill and Botley Hill -	57	11	36 36,25	36
St. Ann's Hill and Hanger Hill	53	22	39,5 40	39,75
Botley Hill and Leith Hill -	108	50	47,5 48,25 51,5	49
Leith Hill and St. Ann's Hill -	77	37	33,75 37,25	35,5
King's Arbour and St. Ann's Hill -	25	15	42 42,5 42,5	42,25
Shooter's Hill and Hanger Hill -	62	57	20 24	22
Leith Hill and Shooter's Hill -	166	2	23,5 23,5	23,5
At Leith Hill.				
Banstead and Botley Hill -	31	21	8 12	10
Banstead and Hind Head - -	140	28	13,5 13,75	13,5
Hind Head and Chanctonbury Ring -	72	56	49,5 51,25	50,25
Ditchling Beacon and Chanctonbury Ring	32	43	56,25 58,5	57,5
St. Ann's Hill and Hind Head -	82	8	51	
Hind Head and Crowborough Beacon	143	57	47,5 47,75	47,5
Hind Head and Bagshot Heath -	56	37	29,5	

Between	°	′	″	Mean. ″
Shooter's Hill and Nettlebed -	86	23	24 27,5	25,75
Hind Head and Shooter's Hill -	148	28	30 32,5 33,25 33,25 33,75	32,5

At Shooter's Hill.

	°	′	″	Mean. ″
Botley Hill and Banstead - -	37	8	25,75	
Banstead and Blackheath -	42	52	48,5	
Hanger Hill and Blackheath -	11	51	1,25	
Leith Hill and Blackheath -	48	50	6 7,5	6,75
Nettlebed and Blackheath - -	7	58	25,5	
Nettlebed and Leith Hill -	56	48	30 32	31
St. Ann's Hill and Blackheath -	12	41	15,75 17,25	16,5

At Bagshot Heath.

	°	′	″	Mean. ″
St. Ann's Hill and Hind Head -	101	49	23,75	
St. Ann's Hill and Leith Hill -	53	52	13,5	
Leith Hill and Hind Head - -	47	57	7 7	7
Nettlebed and Leith Hill -	168	32	12 13 16	13,75
Nettlebed and Highclere - -	60	10	26 22	24
Nettlebed and Penn Beacon -	42	50	12,25 12,75	12,5
Leith Hill and Highclere - -	131	17	22,5	

At Hind Head.

Between	°	′	″	Mean. ″
Nettlebed and Leith Hill - -	94	9	57,5 57,75	57,5
Nettlebed and Bagshot Heath -	18	44	31,25 33,25	32,25
Leith Hill and St. Ann's Hill - -	51	10	38 41,5	39,75
Leith Hill and Rook's Hill -	111	57	2 4,5	3,25
Leith Hill and Butser Hill -	156	25	10,75 8,25	9,5
Leith Hill and Chanctonbury Ring	61	52	25,5	
Chanctonbury Ring and Rook's Hill	50	4	37	
Nettlebed and Highclere - -	43	8 9	58,5 0,5	59,5

At Rook's Hill.

Chanctonbury Ring and Butser Hill	147	49	26,5	
Chanctonbury Ring and Hind Head -	82	42	45 46,5	45,75
Chanctonbury Ring and Dunnose -	137	16	48,5	
Chanctonbury Ring and Beachy Head	14	17	34	
Chanctonbury Ring and Motteston Down	153	1	1	

At Butser Hill.

Rook's Hill and Hind Head - -	70	25	13 14,5	13,75
Rook's Hill and Dunnose - -	80	21	58	
Rook's Hill and Motteston Down -	101	7	7 9	8
Rook's Hill and Highclere -	154	56	56 58,5	57,25
Rook's Hill and Dean Hill - -	156	34	14	*dubious.*

At Chanctonbury Ring.

Between	°	′	″	Mean. ″
Rook's Hill and Leith Hill - -	92	23	25 25,25	25
Rook's Hill and Hind Head -	47	12	37 39,25	38
Hind Head and Leith Hill - -	45	10	46	
Rook's Hill and Ditchling Beacon -	179	8	4 8	6

ART. V. *Further Particulars respecting the Operations of the Year* 1792.

Excepting the stations Nine Barrow Down, Black Down, Wingreen, Long Knoll near Maiden Bradley, Beacon Hill, Inkpin Beacon, with those about the base of verification, all the stations which constitute the series hereafter given, were selected this year.

From an opinion which we entertain, that triangles, whose sides are from 12 to about 18 miles in length, are preferable for the general purposes of a survey, to those of greater dimensions, we have endeavoured to select such stations as might constitute a series of that description. In those which were chosen to the eastward of Bagshot Heath, Hind Head, and Butser Hill, we have in some degree succeeded; but, from local circumstances, we have not been equally fortunate with those to the westward. Instead of Dean Hill, it was hoped that the ground upon which *Farley Monument* stands, might have suited our purpose; but the wood to the west of the hill was found to be so high, that even with the whole stage, the

instrument would not be sufficiently elevated. There remained, therefore, no other expedient but fixing upon Dean Hill, which is the highest spot near Farley Monument. It must be also observed, that Highclere is the only situation which affords the means of carrying on the triangles from the side Bagshot Heath and Hind Head, without forming a quadrilateral.

When the instrument was at Shooter's Hill, a staff was erected on Blackheath, for the purpose of enabling us to determine the direction of the meridian with respect to Nettlebed. This, however, was not done, the weather proving too unfavourable; but as some of the stations were referred to this staff, it may be proper to observe, that on account of its being so near Shooter's Hill, a small portfire was placed in a groove cut in it, which afforded the means of taking an angle very exactly, as the light had the appearance of a bright point.

The interior stations which were selected for the use of the small instrument, were Bow Hill, near Rook's Hill; Portsdown Common, on the road to Portsmouth; and Sleep Down, near Steyning. To the first and last of these the instrument was taken, for the purpose of fixing such objects as could not be intersected from the principal stations. The points on the coast were particularly wanted, for the construction of some maps which were making for the use of the Board of Ordnance. Those places so fixed will be given hereafter; but it must be observed, that few opportunities were lost of searching for church towers, and other objects whose situations were to be determined. That the bearings of those might be taken with precision, the observations were made either in the morning or evening, when the air was free from vapour, and with-

out that quivering motion, which, in summer, it generally has in the middle of the day.

ART. VI. *Improvement in the Axis of the great Theodolite; and the Progress of the Survey in the Year* 1793.

Towards the conclusion of the last year's operation, it was found that the axis of the instrument, by the frequent use of it, was considerably worn, and which was, perhaps, increased by the motion of the carriage, as the arch could not be clamped with tightness sufficient to prevent the circle from moving within the limits of the bell-metal arms, and the upright part of the travelling case. The consequence was, that it sometimes became necessary to let the circle lower by means of the screws; and as it was found to be exceedingly difficult to turn them equally, and by a quantity which was just sufficient, an application was made to Mr. RAMSDEN to apply something to the axis, which might enable us to adjust the circle with greater ease and accuracy. Accordingly, upon the party arriving in town, the instrument was taken to his house, and left there for the winter, during which he made the desired alteration.

The progress made in the survey during the last season, determined the extent of the business for this year: and it was then imagined, that with good weather, we might be enabled to join the triangles to the eastward with those of General ROY, and likewise observe the remaining angles in the series, having first made the necessary observations at Dunnose and Beachy Head for obtaining the directions of the meridian. It had also been foreseen, that it would soon become necessary to select some spot for the measurement

of a new base, not only to verify the triangles remote from Hounslow Heath, but likewise to determine the sides of those which might be hereafter projected for the survey of the west of England. The situation which we had looked forward to, as being the only one which would afford a base line of sufficient extent, was Sedgemoor in Somersetshire, not having then imagined that any place could be found fit for the purpose to the eastward of that situation.

By maturely deliberating upon the steps to be taken for this necessary business, it soon appeared, that Sedgemoor, from its remoteness, would not suit for a base, which was intended to be applied as a test to the sides of the great triangles which were now constituted. Inquiry was therefore made after a spot which might be less exceptionable; and as information was obtained that Longham Common, near Poole in Dorsetshire, was likely to afford such a base, we examined it in the January of this year; but not finding it fit for the purpose, we proceeded to Salisbury Plain, where we found that a base line of nearly seven miles might be measured without much difficulty between Beacon Hill, near Amesbury, and the Castle of Old Sarum. With respect to the nature of the ground, as any observations concerning it will be introduced with more advantage when we treat of the particulars of the measurement, it will be only necessary to observe, that prior to determining upon the possibility of measuring it with the necessary accuracy, we considered of the errors which would be likely to creep in from the many hypotenuses which the base would consist of, and from other circumstances which the ground from its inequality might be supposed to produce.

As the principal object of this year's business was, to deter-

mine the directions of the meridians, the party left London for the Isle of Wight early in the month of March, that it might arrive at Dunnose in proper time for making the required observations. The instrument, however, was first taken to Motteston Down, for the purpose of intersecting many places whose bearings had been last year taken when the instrument was at Rook's Hill, and which were now wanted by the surveyors of the Ordnance. This station had been selected for that purpose, and was never intended to become a principal one in the series; but when the instrument was on the spot, it was considered as proper that some observations should be made to the stations which were at that time chosen. For this reason, when the time for observing the star approached, and most of the lights had been fired without our having seen them, it was not considered of consequence to remain there any longer, and the instrument was therefore taken to Dunnose.

A small staff, of about three inches diameter, was erected on Brading Down, which is about six miles from the station, for the purpose of referring the star to it; a small lamp of six inches diameter, constructed upon the same plan as the large ones, being, when made use of, buckled at the bottom of the staff.

As the best method of obtaining the direction of the meridian, is by observing the star upon each side of the pole, whence the double azimuth is nearly obtained without any correction for the star's apparent motions, every opportunity was watched, of observing it at the times of its greatest apparent eastern and western elongations. But in the unsettled season of the month of April, when almost every wind brought

a fog over the station, many days elapsed without our seeing either the star or staff; and it was on that account we continued so long at Dunnose.

As the truth of the deductions must entirely depend on the accurate determination of the directions of the meridians, the greatest care was taken in making the observations. An hour, and generally more, before the star came to its greatest elongation, the observers repaired to the tent for the purpose of getting the instrument ready. The method of adjusting it, was first by levelling it in the common way with the spirit level which hangs on the brass pins; and afterwards, by that which applies to the axis of the transit. The criterion which determined the instrument to be properly adjusted, was the bubble of the latter level remaining immoveable between its indexes, while the circle was turned round the axis.

As the star, four minutes either before or after its greatest elongation, moves only about a second in azimuth, the time was shown sufficiently near, by a good pocket watch, which was regulated as often as opportunities offered. When the star was supposed to be at its greatest elongation, the observer, if at night, brought it upon the cross wires, and bisected it, leaving equal portions of light on each side of the cross: but if it was in the day, when the star appeared like a point, the telescope was moved in the vertical till it came near the vanishing point of the cross. At either of these times, when the observer was satisfied of the star being properly bisected, or brought into the vanishing point formed by the wires, another person who had kept his eye at the microscope, bisected the dot. The transit was then taken off, and the instrument being turned half round, and the telescope replaced, the star

was observed again. This precaution was taken to obviate the errors which might arise, from the arms of the instrument being out of the parallel with the plane of the circle, owing to any imperfections in the position of the Ys, on which the transit rested. It was, however, seldom found, that a greater difference subsisted between the readings of the opposite microscopes, than what might be supposed to be the consequence of a shake in the centre, or errors in division. A mean of the readings was always taken. It must be also mentioned, that out of twenty, three and four inch white lights, which were fired at Beachy Head, only three of them were seen: but the angle between that place and the staff on Brading Down was considered, from the near agreement in the observations, to be determined with the necessary accuracy.

After the business was finished at Dunnose, the instrument was taken to Chanctonbury Ring, and Ditchling Beacon; and from the latter place to Beachy Head, in order to observe the direction of the meridian; but after placing a staff upon the high ground above Jevington, we were obliged to defer the attempt, as it was found, that owing to the effects of heat, the air was not sufficiently steady for the staff to be seen distinctly, when the star came to its greatest elongation in the day time, if the sun shone out. We therefore left Beachy Head, and proceeded to the following stations, viz. Fairlight Down, Brightling, Crowborough Beacon, and Botley Hill; from which latter place we returned in June to Beachy Head, and observed the direction of the meridian.

From this station, the party went to Dean Hill, and thence to Salisbury Plain, for the purpose of fixing on the extremities of the new base. This being done, the instrument was taken

to Old Sarum, Four Mile Stone, Beacon Hill, Thorny Down, and Highclere, where the operations of this year terminated. But it must be observed, that owing to a strain which the clamp of the instrument sustained when at Thorney Down, no dependance could be placed on the observations which were made at Highclere. Upon this being discovered, and the season too far advanced to permit of any business being done after the instrument might be repaired, the party returned to London.

ART. VII. *Angles taken in the Year* 1793.

At Motteston Down.

Between	°	′	″	Mean. ″
Nine Barrow Down and Dunnose -	159	51	2,5 5	3,75
Butser Hill and Dunnose - -	64	41	2	
Rook's Hill and Dunnose -	44	57	46 *dubious.*	

At Dunnose.

	°	′	″	Mean. ″
Dean Hill and Brading staff -	55	58	38,5 38,75	38,5
Motteston Down and Brading staff	94	49	19	
Nine Barrow Down and Brading staff	109	11	3,5 8	5,75
Butser Hill and Brading staff - -	0	15	31,5	
Rook's Hill and Brading staff -	24	28	42,5 45,5	44
Chanctonbury Ring and Brading staff	40	11	44	
Beachy Head and Brading staff -	60	42	40 42 42,25	41,5

Between		°	′	″	Mean.
Pole star and Brading staff	Apr. 21, aftern.	24	4	21,25	
	22, aftern.	24	4	22	
	28, aftern.	24	4	23	
	29, morn.	18	24	0	
	May 5, aftern.	24	4	27,25	
	12, aftern.	24	4	29,5	
	13, morn.	18	23	53,25	
At Chanctonbury Ring.					
Beachy Head and Shoreham staff -		32	49	48,5	
				49	
Dunnose and Shoreham staff -		98	9	48,75	49,25
				49,75	
Rook's Hill and Shoreham staff -		125	10	2,25	
At Ditchling Beacon.					
Beachy Head and Lewes staff -		20	52	0,75	
Crowborough Beacon and Lewes staff		57	8	36	
Leith Hill and Lewes staff -		135	27	1,75	3
				4	
Brightling and Lewes staff - -		25	40	18,25	
Chanctonbury Ring and Lewes staff		164	1	31	32,25
				32,5	
				33,5	
At Fairlight Down.					
Brightling and Beachy Head -		59	33	1,5	1,75
				2	
At Brightling.					
Fairlight Down and Beachy Head		80	44	17,5	19,25
				21	
Crowborough Beacon and Beachy Head		102	58	14	15,5
				17	

Between	°	′	″	Mean. ″
Ditchling and Beachy Head -	59	29	13,5 14,5	14
At Crowborough Beacon.				
Brightling and Leith Hill -	168	27	20,5 22	21,25
Brightling and Ditchling Beacon	105	2	43 44,75	44
Brightling and Botley Hill -	145	20	27	
At Botley Hill.				
Banstead and Wrotham Hill -	152	57	2,5 6	4,25
Banstead and Shooter's Hill -	85	39	58,5	
Banstead and Crowborough Beacon	129	23	3,5	
Crowborough Beacon and Leith Hill	89	35	1	
At Beachy Head.				
Brightling and Jevington staff	46	59	33,25 34,75	34
Fairlight Down and Jevington staff	86	42	12 14	13
Rook's Hill and Jevington staff -	48	39	59	
Chanctonbury Ring and Jevington staff	40	57	21 23	22
Dunnose and Jevington staff -	69	26	51,25 52 52 53,25	52
Ditchling Beacon and Brightling -	73	58	25 28	26,5
Pole star and Jevington staff, Jul. 15 at night	30	19	54,5	
16 night	30	19	57,5	

Between		°	′	″	Mean. ″
	Jul. 26 at morn.	24	38	19	
	30 night	30	19	50,5	
	Aug. 1 morn.	24	38	20,25	
	1 night	30	19	49,5	
	2 night	30	19	50,25	
	3 morn.	24	38	23,5	
	* 11 night	30	19	47,25	

At Dean Hill.

	°	′	″	Mean.
Beacon Hill and Highclere -	50	18	47,5 47,5	47,5
Beacon Hill and Wingreen - -	82	56	47 50	48,5
Beacon Hill and Dunnose -	160	46	8,5	
Beacon Hill and Nine Barrow Down	134	23	32,25 32,75	32,5
Beacon Hill and Motteston Down	174	34	56,5 58,5	57,5
Beacon Hill and Four Mile Stone	39	29	1,5 5	3,25
Beacon Hill and Butser Hill -	112	41	36 36,5 38	36,75

At Old Sarum.

	°	′	″	Mean.
Beacon Hill and Four Mile Stone	85	58	21,5 21,75 22,25 23,75	22,5
Beacon Hill and Thorney Down -	48	26	3 4,25 6,5	4,5

* Many observations of the star at this station, and also at Dunnose, are rejected on account of their being made under unfavourable circumstances.

At Four Mile Stone.

Between	°	′	″	Mean. ″
Beacon Hill and Old Sarum -	70	1	45,75 47,25 48,25 49	47,5
Beacon Hill and Dean Hill - -	72	4	46,5 49,25	48

At Beacon Hill.

Old Sarum and Four Mile Stone -	23	59	50,25 52,25	51,75
Old Sarum and Thorney Down -	33	33	23,75 24 26	24,5
Dean Hill and Four Mile Stone -	68	26	8,5 10,25 11	10
Dean Hill and Highclere -	102	45	23,5	
Thorney Down and Highclere	113	38	13,75 16,75	15,25

At Thorney Down.

Beacon Hill and Highclere -	53	22	28,5 30	29,25
Beacon Hill and Old Sarum -	98	0	29,25 32,5	31

At Highclere.

Dean Hill and Beacon Hill -	26	55	53 54	53,5

ART. VIII. *Particulars relating to the Operations of the Year* 1794.

The party this year took the field the fourth of March, and proceeded from London to the Isle of Purbeck, taking Butser

Hill in its way. In the observations of the year 1792, the angle at that station, between Rook's Hill and Dean Hill, is noted to be dubious. The reason which induced us to be of that opinion was, that the telescope, by some accident, was thought to have been moved after the observation of the light, and just at the time when the angle was about to be read off. As the season was then far advanced, and four lights had been fired, without our having seen more than one of them, it was determined to leave the final observation of that angle till this year. Accordingly upon our arrival at Butser Hill this second time, a lamp was sent to each of the stations, and the angle repeatedly taken, as given in the following article. The party from thence proceeded to Nine Barrow Down in the Island of Purbeck.

The reason of the business commencing so early in the season, arose from the necessity of beginning the measurement of the base on Salisbury Plain, towards the latter end of June, that it might be finished before the year should be far advanced, when the cultivated ground a mile to the northward of Old Sarum would be ploughed. It was also necessary that the angles at Wingreen and Highclere should be observed.

On account of the magnitude of the 24th and 27th triangles, the instrument was kept at the station in the Island of Purbeck till the angles between Dean Hill and the stations in the Isle of Wight were determined very accurately. It was, therefore, not till a month after the two first lights were fired, that as many observations were made as we deemed to be sufficient.

As it will answer our purpose better, to give an account of the stations which were chosen this year, for the further prosecution of the survey, in another part of this work; it remains

only to be observed, that from Nine Barrow Down the instrument was taken to Black Down, near Dorchester, and from thence to Wingreen, Highclere, and Beacon Hill; the observations which were made this year being concluded at the latter place in the beginning of June. It may, however, be mentioned, that in addition to the interior stations chosen in the year 1792, for the future use of the small instrument, three others were selected in this and the preceding season, namely, *Ramsden Hill*, near Christchurch; *Thorness* in the Isle of Wight; and *Stockbridge Hill.*

ART. IX. *Angles taken in the Year* 1794.

At Butser Hill.

Between	°	′	″	Mean. ″
Rook's Hill and Dean Hill -	156	34	19,75 20,5 19,75	20

At Nine Barrow Down.

	°	′	″	Mean. ″
Dean Hill and Wingreen -	39	34	27,75 30,25 28,5	28,75
Dean Hill and Motteston Down -	56	9	55 55,5	55,25
Dean Hill and Dunnose - -	61	57	20,75 20 19	20
Lulworth and Bull Barrow -	52	47	34,25 32	33
Dean Hill and Bull Barrow -	71	31	55,5 56,5 53 52	54,25

Between	°	′	″	Mean. ″
Black Down and Bull Barrow -	38	58	19	19,25
			19,5	
At Black Down.				
Lyme and Bull Barrow -	124	32	33,25	33,25
			33,25	
Bull Barrow and Nine Barrow Down	56	30	18,25	18,75
			19,5	
			18	
			19,75	
Bull Barrow and Lulworth -	65	35	40,5	42,5
			41	
			42,5	
			45,5	
Lulworth and station above Chesil, in Portland -	42	3	16,25	19,75
			19,75	
			19,75	
			21,75	
			21	
Lulworth and station near Portland Light House -	52	43	49,25	51,75
			51,25	
			53,25	
			53,25	
Pilsden Hill and Mintern - -	66	51	19,25	21,75
			21	
			24,75	
Mintern and Bull Barrow - -	31	25	56,75	57,5
			57	
			59	
At Wingreen.				
Beacon Hill and Dean Hill - -	30	13	23,75	23
			22	
			23,5	

Between	°	'	"	Mean. "
Dean Hill and Nine Barrow Down	88	58	45,25	46,5
			47,75	
Dean Hill and Bull Barrow -	143	28	21	23
			22	
			23,75	
			25,25	
Bull Barrow and Bradley Knoll -	96	20	39,25	37
			36,5	
			33,25	
			38,25	
			37,25	
Bradley Knoll and Beacon Hill -	89	57	40,25	37,75
			37,75	
			37,75	
			37,25	
			35	
At Highclere.				
Butser Hill and Dean Hill - -	69	8	33,5	35
			36,75	
			35	
Dean Hill and Beacon Hill - -	26	55	50,5	51,5
			52,25	
Thorney Down and Beacon Hill -	12	59	10,5	10
			9,25	
Beacon Hill and Inkpin Hill - -	56	0	29	29,75
			30,25	
			30	
Beacon Hill and White Horse Hill (near Wantage) - - -	90	28	20	20,5
			21	
Nuffield and Bagshot Heath -	46	10	17,5	18,5
			19,5	
Bagshot Heath and Hind Head - -	34	46	14,75	15,75
			15,75	
			16,75	

Between	°	′	″	Mean.
Butser Hill and Hind Head - -	29	12	22 22,$\frac{1}{4}$	22
At Beacon Hill.				
Dean Hill and Wingreen - -	66	49	52,25 51,75	52
Wingreen and Bradley Knoll -	32	11	44,75 44,25 43,5 40,75	43,25
Inkpin Beacon and Dean Hill -	120	28	2,25 1,25 3	2
Wingreen and St. Ann's Hill (near Devizes) - - -	106	27	9 8 7	8

ART. X. *Situations of the Stations.*

Hanger Hill. The station on this Hill is in the field to the eastward of the Tower, and within 13 feet of the eastern hedge. The Tower bears due west of the station.

Shooter's Hill. The station is in the north-west corner of the field, opposite to the Bull Tavern.

Banstead. The station is in a field belonging to Warren Farm, near the road leading to Ryegate. It is fourteen feet north of the hedge, and may be easily found, as Leith Hill and an opening between two rows of trees on Banstead Common, are in a line with the station.

Leith Hill in Surrey. The station is 32 feet from the north-east corner of the Tower, and in that direction from it.

Crowborough Beacon, Sussex. The station is about 600 feet

due south of the spot on which the beacon was formerly erected.

Brightling, Sussex. The station is about 70 feet south-west of the gate belonging to the field in which stands Brightling Windmill.

Beachy Head. Twelve yards south-west of the Signal-house. The muzzle of the gun is above the surface of the ground.

Ditchling Beacon, Sussex. The station is in the middle of a small rising, which has the appearance of having once been a Barrow.

Chanctonbury Ring, Sussex. This place is near Steyning; and the station is situated 50 feet from the ditch on the west side of the Ring.

Rook's Hill, near Goodwood, Sussex.. The station is east of the Trundle, and near it.

Butser Hill, Hampshire. There is no precise way of pointing out the spot on which the instrument was placed: the general situation of it, however, may be known: it is on the middle of the hill, which is itself near, and to the northward of the Fifty-four Mile-stone on the Portsmouth road.

Dunnose, Isle of Wight. The station is 87 feet northward of Shanklin Beacon: the muzzle of the gun is above the surface of the ground.

Motteston Down, Isle of Wight. The station is on the west Barrow.

Nine Barrow Down, Isle of Purbeck. The station on the highest of the *Nine Barrows*.

Black Down in Dorsetshire. The station is 23 feet west of the North Barrow. Black Down is six miles from Dorchester, and near the village of Winterbourn.

Bull Barrow Hill, near Milton Abbey in Dorsetshire. The station is on the Barrow.

Wingreen, Dorsetshire. The hill so named, is four miles east of Shaftesbury, and the station is about 80 feet south-west of the Ring, or clump of trees.

Beacon Hill, about two miles from Amesbury, near the Andover road, Wiltshire. The station may be easily found, as there is a stone whose surface is above that of the ground, placed about 10 feet east of it.

Old Sarum. The station is south-east of the Two Mile-stone, and near it. A large stone with its surface above that of the ground, is placed 11 feet due west of the station.

Four Mile-stone, Wiltshire. The station is in the field west of the Four Mile-stone on the Devizes road, leading from Salisbury. It is on the rising which is in the middle of the field.

Thorney Down, Wiltshire. The Down is near Winterbourn, and the station to the north of the wood.

Dean Hill, Hampshire. This place is near the village of Dean, and about 6 miles east of Salisbury: the station is in the north-west corner of a field belonging to Mr. HALIDAY.

Inkpin Beacon, Wiltshire. This hill is above the village of Inkpin, and the station is in the centre of the small field circumscribed by a ditch and parapet of an ancient fortification.

Highclere, Wiltshire. The station is in the centre of the Ring on Beacon Hill, about half a mile south-east of Highclere.

Bagshot Heath. The station is on the brow of an eminence two miles north of the Golden Farmer, and directly west of the north corner of Bagshot Park.

Hind Head, Surrey. The station is near the Gibbet, being about 22 feet north-west of it.

The situations of those stations which are common to this operation and that of General Roy, are not described, the same being done in the LXXXth Volume of the Philosophical Transactions.

As it is probable that some individual will avail himself of the particulars given in this performance, by forming more correct maps of the counties over which the triangles have been carried, and who consequently may wish to visit certain of the stations, it is proper to observe, that small stakes are placed over the stones sunk in the ground, having their tops projecting a little above it. For some years there will be little difficult yin finding the stations, as the spots are well known to the neighbouring inhabitants.

SECTION THIRD.

Measurement of the Base of Verification on Salisbury Plain *with an Hundred Feet Steel Chain, in the Summer of the Year* 1794.

ART. I. *Apparatus provided for the Measurement, and the Method of using particular Articles of it.*

The apparatus with which this base was measured arrived at Beacon Hill the 25th of June, and consisted of the two steel chains, the tressels belonging to the Royal Society, and the twenty coffers which were used on Hounslow Heath, together with the pickets, iron-heads, and a few other articles, which in the beginning of this year had been made at the Tower. As it was foreseen that the truth of this measurement would, in a great degree, depend on the accurate reduction of the several hypotenuses to the plane of the horizon, an application was

made to Mr. RAMSDEN in the foregoing winter, to consider of some means by which their inclinations might be obtained. He therefore applied an arch S to the side of the transit telescope, as exhibited in Tab. XLIII. which he divided into half degrees; and opposite to this he placed a microscope T, with a moveable wire in its focus, by means of which, and the micrometer of the telescope, an angle could be taken.

On the first convenient opportunity after the arrival of the apparatus, we determined the value of any number of revolutions of the micrometer-screw in parts of a degree, by the following method.

At the distance of an hundred feet from the transit, a picket was set up, on which a dot was made with chalk, and the instrument being adjusted, was moved by the finger-screw till the edge of the micrometer-wire touched some prominent part of that mark. The wire in the focus of the microscope was then made to bisect a dot upon the arch, and the telescope moved in the vertical till the next dot was bisected, by which the instrument had described half a degree upon its axis, and the micrometer-wire was afterwards moved till it touched the same part of the chalk mark, the revolutions being counted, which were consequently equal to thirty minutes. This operation was repeatedly tried, with a picket placed from one to six hundred feet successively from the telescope, the runs of the micrometer-screw being in each case nearly the same, as indeed they ought to be according to theory.

The number of revolutions equal to $30'$ was found, from a mean of these trials, to be $12\frac{10}{100}$.

Having determined this, the chains A and B were compared with each other, when they were found to have the same difference of lengths as when measured by Mr. RAMSDEN.

For the purpose of tracing out the line of the base, as Beacon Hill had a commanding view of almost the whole of it, the instrument was kept in the tent after the observations were finished: and at different times, when the air was sufficiently steady for the purpose, many points in the true direction were found by bisecting the staff erected at Old Sarum, and moving the transit in the vertical, whilst a person placed a camp-colour in the proper situation on the ground, by means of signals which were made at Beacon Hill.

As it appeared, when this spot was first selected for the measurement, that in the course of it there would be frequent necessity for changing the directions of the hypotenuses, a brass bar, of a prismatic form, had been provided, by means of which, and a plumb-line, a new direction was easily taken. The method of using them was as follows.

A picket was driven into the ground close to the handle of the chain, having its top eight or ten inches above the place where the preceding hypotenuse was to terminate, one of the register-heads, with the bar, being screwed on it. The chain was then stretched, and the silver wire, or plumb-line, made to pass through the handle, whilst the slider was moved till the wire came upon the dart, marking by this means, the termination of the hypotenuse. In order, however, to give a more perfect idea of this matter, a figure is given in Tab. XLV. where B is the bar, with the wire falling through the handle of the chain, one half of it being left out, for the purpose of showing its coincidence with the arrow on the handle.

The experience which we had obtained in the measurement of the base on Hounslow Heath, led us to discover, that some of the methods we made use of to execute particular parts of it, might have been improved. One of them was, the means

by which the heads of the pickets were placed in the plane of the base, which frequently was the cause of the planes of the register-heads being out of the direction of the hypotenuses. In this operation, however, the bottoms, as well as the tops of them, were placed in the true vertical by means of the transit-instrument, and therefore it was not difficult to bring the planes of their tops into the required position.

For the purpose of using the transit as a boning telescope, as well as an instrument for taking the angles of elevation or depression, Mr. RAMSDEN provided two mahogany boards, one of which was fastened to the register-head, and the other (furnished with levelling screws) rested upon it, the transit-instrument being placed on the latter.

The level belonging to the transit was then hung on the arms; and if the axis proved to be horizontal, which it would be if the brass heads were rightly placed, the instrument required no farther adjustment; but if that did not prove to be the case, the axis was made parallel to the horizon by the screws of the levelling-board, which were turned in contrary directions, having in the first instance been worked till within half the limits of their adjustment. By this means the axis was kept at a constant height from the brass heads.

A board with a cross piece, whose upper edge from the bottom of it was equal to the distance of the axis of the instrument from the head of the picket, was placed on another picket which had been driven till its head was at a convenient height in the plane of the base, and the transit moved in the vertical till the edge of the wire in the centre of the glass, coincided with that of the cross piece. The rest of the pickets in that hypotenuse were then driven into the ground, till their tops

were in the same right line, as discovered by the application of this board to their heads.

The method of determining the angles which the measured lines made with the plane of the horizon was as follows.

After the hypotenuse was measured, the transit-instrument with its boards were placed on the picket, and the levelling-screws moved as before described, if the axis did not happen to be horizontal. The cross board, upon which a black line was drawn whose breadth was about twice the apparent thickness of the micrometer-wire, and its distance from the bottom of it equal to that of the axis of the instrument from the register-head, was placed on another picket in the hypotenuse, having the brass head which had been before fixed on it still remaining. The telescope was then made horizontal, the index of the micrometer being placed to the *zero* on its circle, and the wire of the microscope set to bisect that dot on the arch which was nearest to the centre of the field. After this, the telescope was moved in the vertical by the finger-screw, till another dot was bisected, at the same time that the line upon the cross board appeared in the glass, by which the angle that the instrument had described on its axis, was measured in half degrees. The remaining part of the angle, or rather the fractional part of an half degree, was measured by the micrometer, the wire of which was brought from the centre of the glass to bisect the black line, and was either added to, or subtracted from, the former quantity, as the angle described by the telescope fell short of, or exceeded, that formed by the hypotenuse and the plane of the horizon.

By this method, all the angles of elevation and depression were taken. And we consider it as probable that they are

within a quarter of a minute of the truth; since the instrument was capable of being used with great accuracy, the arch having been divided by one of Mr. RAMSDEN's best workmen, and the value of one, or any number of revolutions of the micrometer-screw, had been accurately obtained. If, therefore, any considerable errors have taken place in this part of the operation, they must have arisen from the axis of the transit-instrument and the line on the cross board not being of the same height from the brass heads on which they were placed: but we think there is almost a certainty that this difference was confined to such limits as will not introduce any errors of consequence; for even supposing the register-heads were placed on the pickets so unskilfully that it became necessary to turn the screws on the levelling-board as much as they were capable of, whilst the third remained unmoved, in order to adjust the transit, the error introduced on that account would be only half a minute, even though the hypotenuse should consist of but one chain, and be inclined to the horizon eight degrees. We therefore think ourselves justified in the opinion which we entertain of these angles being determined with sufficient accuracy; since, if an error of one minute had taken place in the inclination of each hypotenuse, and those errors lay all one way, the length of the base, as hereafter given, would only be varied three inches by that circumstance.

It may, perhaps, be imagined that some small errors have arisen from the handle of the chain not lying flat upon the brass heads when the new directions have been commenced. To obviate this, precautions were always taken to drive the pickets at the termination of the hypotenuses in such a manner, that the arrow on the handle could be made to coincide

with one of the divisions near the end of the brass scale, by which any error arising from their not being exactly in the same vertical plane, was rendered so trifling as not to be worth notice.

Having now related, with as much conciseness as the subject will admit, the methods which were adopted for the execution of the most essential parts of this operation, there remain only a few other particulars to be related before we give the reduction of the base.

After as many points as were judged necessary had been fixed in the true direction, by the means heretofore described, and the chains compared with each other, the mensuration was begun, and continued without much interruption for seven weeks, when it was finished with that part of the 366th chain which terminated its apparent length.

The method taken to mark this last mentioned chain, was by cutting a small hole in the bottom of the coffer, through which a plumb-line was made to pass, the point of the plummet being brought over the end of the base, and the chain moved till it touched the wire; a slight scratch was then made with a file at the point of contact.

On the first favourable opportunity, subsequent to this conclusion of the measurement, the chains A and B were compared with each other, when it was found that the wear of the former, by the constant use of it, was only one division of the micrometer head, or $\frac{1}{200}$th of an inch. The smallness of this quantity in the measurement of a base of such great length, was doubtless owing to the pivots, and pivot holes of the joints being smoothed, and as it were polished, in the operation on Hounslow Heath; and it may also be adduced as some proof,

that the joints had not rusted while the chains remained in the Tower; but to prevent this, care had been taken to deposite them in a dry place, being afterwards frequently examined and oiled.

Thus concluded the measurement of this base, in which it is certain that great pains were taken to produce an accurate result; and we are not without hopes, that the many obstacles which offered themselves have been surmounted with success; but this is left to the decision of the candid and intelligent reader.

The following table contains the particulars of this operation. The first column showing the number of hypotenuses; the second, that of the chains in each hypotenuse; the third, the observed angles of elevation or depression given to the nearest 10″; the fourth and fifth, the perpendiculars answering to the elevations and depressions; the sixth, the reduction of the hypotenuses to the horizontal lines, or the versed sines of the elevations and depressions to the hypotenuses as *radii*; the seventh and eighth, the perpendicular distance between the termination and beginning of any two hypotenuses when a new direction was commenced above or below.

ART. II. Table of the Measurement of the Base of Verification.

Hypotenuses. No.	Hypotenuses. Chs.	Angles of Elev. or Depr. ° ′ ″	Perpendiculars. Elevation. Feet.	Perpendiculars. Depression. Feet.	Reduction. Feet.	Below. Inches.	Above. Inches.
1	1	7 52 30		13,7012	0,9431		
2	1	11 31 40		19,9843	2,0172		
3	1	10 5 0		17,5080	1,5446		
4	1	7 25 20		12,9180	0,8379		
5	1	5 41 50		9,9272	0,4940		
6	7	4 49 30		58,8788	2,4806		
7	6	4 18 40		45,1033	1,6977		
8	3	3 48 30		19,9257	0,6625	31,5	
9	3	3 13 0		16,8336	0,4727	21,5	
10	1	0 9 0		0,2618	0,0003		
11	1	2 27 30	4,2893		0,0920		
12	1	0 58 30	1,7016		0,0145		
13	3	0 5 0	0,4363		0,0003		
14	6	0 34 10		5,9631	0,0293	11,5	
15	1	3 9 10	5,4999		0,1514		
16	2	1 25 20	4,9640		0,0616		
17	2	0 24 10	1,4059		0,0049		
18	5	0 8 10		1,1878	0,0014		
19	4	0 49 10	5,7206		0,0409		
20	4	0 10 50	1,2605		0,0020		
21	3	1 19 20		6,9225	0,0799	7,0	
22	7	1 38 20		20,0201	0,2864		
23	5	1 33 40		13,6216	0,1856	5,5	
24	6	1 18 20		13,6706	0,1558	14,5	
25	1	1 34 30		2,7485	0,0378		
26	9	1 15 0		19,6334	0,2142		
27	6	1 0 50		10,6169	0,0939		
28	2	0 5 40	0,3297		0,0003		
29	3	0 49 50	4,3486		0,0315		
30	5	0 15 10	2,2059		0,0049		
31	3	0 18 20		1,5999	0,0043		
32	5	0 8 50	1,2848		0,0017	18,5	
33	3	0 53 30	4,6686		0,0363		
34	8	0 8 50	2,0556		0,0026		
35	10	0 45 10	13,1381		0,0863		
36	4	0 14 0		1,6290	0,0033		
37	5	0 52 0		7,5628	0,0572		
38	2	1 40 10		5,8266	0,0849		
39	7	0 35 30		7,2284	0,0373		
40	4	1 3 10		7,3494	0,0675		
41	3	0 33 50		2,9525	0,0145	19,25	
42	1	0 54 10	1,5756		0,0124		
43	2	1 37 0	5,6425		0,0796		

Hypotenuses. No.	Chs.	Angles of Elev. or Depr.	Perpendiculars. Elevation.	Perpendiculars. Depression.	Reduction.	Below.	Above.
		° ′ ″	Feet.	Feet.	Feet.	Inches.	Inches.
44	3	0 8 40		0,7563	0,0009		
45	3	0 50 10		4,3777	0,0319		
46	4	0 55 50		6,4962	0,0529	20,0	
47	11	0 31 40		10,1325	0,0467		
48	3	0 45 30		3,9705	0,0263		
49	3	1 18 40		6,8644	0,0785		
50	2	1 58 50		6,9121	0,1195		
51	2	3 49 30		13,3418	0,4455		
52	2	3 24 20		11,8806	0,3532	29,25	
53	2	3 20 50	11,6774		0,3412		
54	2	2 31 10	8,7917		0,1933		
55	2	1 7 0	3,8976		0,0380		24,5
56	7	0 25 40		5,2262	0,0195		
57	5	0 55 40		8,0960	0,0656		
58	2	3 2 50		10,6318	0,2828		
59	2	5 34 10		19,4104	0,9441		
60	1	2 4 50		3,6305	0,0659		
61	4	0 34 10	3,9754		0,0198	8,5	
62	2	0 51 40		3,0057	0,0225		
63	3	1 21 40		7,1261	0,0847	33,0	
64	9	3 4 30	48,2788		1,2958		29,0
65	4	2 16 10	15,8396		0,3137		28,75
66	6	0 14 20	2,5016		0,0052		
67	6	1 19 10		13,8160	0,1591		
68	3	1 56 30		10,1646	0,1722		
69	3	0 25 10	2,1962		0,0080		
70	2	0 51 10	2,9766		0,0222		
71	5	0 48 20		7,0296	0,0494		
72	4	0 35 40	4,1499		0,0215		
73	4	1 30 0	10,4708		0,1371		
74	4	1 5 20	7,6014		0,0722		17,5
75	4	0 38 50		4,5184	0,0255		6,0
76	5	1 56 30		16,9410	0,2871	42,0	
77	12	0 34 50		12,1579	0,0616		
78	7	1 8 50		14,0150	0,1403		
79	9	1 37 40		25,5656	0,3632	12,0	
80	3	1 49 40		9,5686	0,1526		
81	4	0 1 0		0,1163			
82	7	1 25 0		17,3061	0,2140		
83	4	1 46 40		12,4092	0,1925		
84	7	0 41 50		8,5180	0,0518		
85	5	0 46 20	6,7387		0,0454		
86	3	0 20 40	1,8035		0,0054		12,0
87	3	1 34 20		8,2311	0,1129		
88	3	3 7 10	16,3253		0,4445		
89	5	1 2 20	9,0655		0,0822		
90	6	0 4 20		0,7563	0,0005		
91	3	1 34 50		8,2747	0,1141	4,0	
92	3	0 21 30	1,8762		0,0059		
			218,6937	634,8222	20,9158	278,0	117,25

ART. III. *Reduction of the Base measured on Salisbury Plain, to the Temperature of 62°.*

	Feet.
The overplus of the 366th chain was measured by Mr. RAMSDEN, and found to be 9,939 feet; therefore the apparent length of the base was -	36590,061
By the measurement in the Duke of Marlborough's riding-house, the chain A was found to exceed 100 feet in the temperature of 54°, by 0,11425 inches; to which adding half the wear, namely, $\frac{1}{520}$ inch, we get $\frac{0,11617}{12}$ feet for the excess of the chain's length above 100 feet; therefore $\frac{0,11617}{12} \times 365,9$ (chains) $=$ 3,542 feet, is the correction for excess and wear; which add - - - -	+ 3,542
The sum of all the degrees shown by the thermometers, was 146051; wherefore $\overline{\frac{140651}{5} - 54° \times 365,9}$ $\times \frac{0,0075}{12} = 5,232$ feet, is the correction for the mean heat in which the base was measured above 54°, the temperature to which the chains were reduced; and this add - - - -	+ 5,232
Hence these corrections, added to the apparent length, give - - - -	36598,835
Again, for the reduction to the temperature of 62°, *viz.* for 8° on the brass scale, we have $\frac{0,01237 \times 365,9 \times 80}{12}$ $= 3,017$ feet; which subtract - -	— 3,017

By the tables, the sum of the versed sines of the

hypotenuses, or the corrections for reducing them to the plane of the horizon, is 20,916 feet;
and this subtract - - - - — 20,916

36574,902

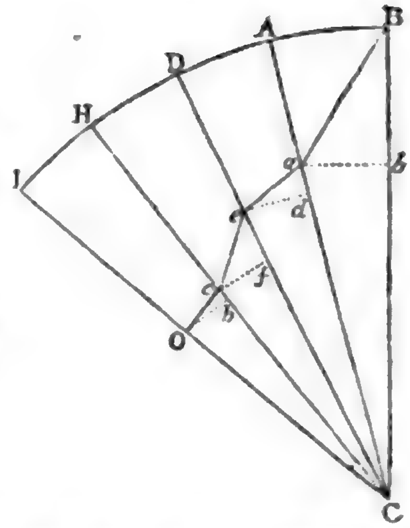
The sum of the corrections, for the reduction of the several horizontal lines from the height of the different hypotenuses above the centre of the earth, to the height of Beacon Hill above ditto, is 0,521 feet; this add - - - + 0,501

Therefore the apparent length of the base, as reduced to the level of Beacon Hill, is - feet 36575,401

But it will be hereafter shown, that the height of Beacon Hill above the sea is 690 feet nearly, and that of King's Arbour 118, and of Hampton Poor House 86 feet; therefore the height of Beacon Hill above the mean point between King's Arbour and Hampton Poor House, is 588 feet, or 98 fathoms.

Now as the base thus reduced, may be supposed to have been measured 98 fathoms farther from the centre of the earth, than that on Hounslow Heath, it must be reduced to the same level. Therefore if we take 3481794 fathoms for the mean semi-diameter, and add 98 fathoms to it, we shall get the length by this proportion, viz. 3481892 : 3481794 :: 36575,4 : 36574,4, the length of the base nearly.

With respect to that step by which the base is reduced to the level of Beacon Hill, or the correction 0,501 foot is obtained, it will be proper to show on what principle it is founded.



In the adjoining figure, let B *a*, *a e*, *e c*, and *c* O be the several hypotenuses, or measured lines; then will the sum of the corrections for their reduction to the plane of the horizon, as given in the table, exhibit that of the differences between the horizontal lines, *b a*, *d e*, *f c*, *b* O, and their corresponding hypotenuses.

Again, with the radius C B, C being the centre of the earth, describe the arc B I, or that subtended by the base, and through the terminations of the several hypotenuses, draw the lines C A, C D, C H, and C I; then will the lines B A, A D, D H and H I be those to which the horizontal ones *b a*, *d e*, *f c*, and *b* O are to be reduced, and which may therefore be done by the proportions of the lines, C *a*, C *e*, C *c*, and C O, to the constant radius C B. Upon this principle, the correction 0,501 foot has been obtained, and which is the sum of the differences between the lines *b a*, *d e*, *f c*, and *b* O, and their corresponding ones in the arc B I.

ART. IV. *Height of Beacon Hill above the Southern Extremity of the Base.*

	Feet.
The sum of the perpendiculars or elevations in the fourth column, is - -	218,6937
And of the depressions in the fifth column	634,8222
Therefore the depressions exceed the elevations	416,1285
The difference of the sums in the seventh and eighth columns, is, in feet - - -	13,35

Hence the sum is the height of the beginning of the first chain above the end of the last, namely, 429,48

But the handle of the chain at Beacon Hill was 6,7 feet above the stone, and at the other end it was 1,3 feet; therefore their difference is 5,4 feet, which subtract - - - - - 5,4

Hence the surface of the stone at Beacon Hill is higher than the surface of the stone at Old Sarum. 424,08

ART. V. *Conclusion of this Section.*

When this situation was first examined, and selected for the measurement, it was imagined that one of the extremities of the base would be fixed on somewhere near the southernmost clump of fir trees, not far from the Amesbury road, because from that spot Highclere can be seen. Those trees are near the 52d hypotenuse, and therefore about a mile from Beacon Hill; consequently, if that situation had been fixed on, the base would have been no more than six miles, and the correction for the reduction of the hypotenuses to the plane of the horizon only about 16 feet.

Now, although we think that the fixing on Beacon Hill as the northern extremity, is justified from the circumstance of a mile being added to the base, which is conceived to be more than a counterbalance for any errors which may arise from measuring down the side of a hill; there were other reasons which made it proper; a principal one is, that by selecting that spot, the base can be applied as a test to the triangles, without making the connection by means of several small ones; and another is, that if a place near the trees had been fixed on, a station must afterwards have been chosen on Beacon

Hill, in order to have a view of Long Knoll, near Maiden Bradley, and Inkpin Beacon towards Hungerford.

We shall now close this section by observing, that the measurement of this base has been almost without an alternative, since Sedgemoor, the only spot west of Salisbury proper for an operation of this kind, is about to be inclosed. Therefore had we not adopted this expedient, the triangles which may hereafter be carried on to the remote parts of the west of England, would probably have depended on the Hounslow Heath base. But we are led to believe, that this base has been measured with nearly the same accuracy which would have attended the operation, had the ground been nearly level; since there is a certainty of the angles, formed by the hypotenuses and the plane of the horizon, being determined within a minute of the truth. Now if an error of a minute in those inclinations, supposing them all to lie the same way, produce only that of three inches in the whole base, it may be concluded that 36574,4 is very nearly its true length.

SECTION FOURTH.

Calculation of the Sides of the great Triangles.

ART. I. *Of the Division of the Series into different Branches.*

In order to methodize the contents of this section, it has been considered as proper to divide the series into different branches, as the triangles of which they are composed seem naturally to resolve themselves into distinct classes.

The first branch, is that which immediately connects the

base of departure on Hounslow Heath, with that of verification on Salisbury Plain, and is bounded by the sides connecting the stations, Hanger Hill, St. Ann's Hill, Bagshot Heath, Highclere, Beacon Hill, and Four Mile-stone on the north, and on the south side by Four Mile-stone, Dean Hill, Butser Hill, Hind Head, Leith Hill, and Banstead.

The second branch, is that which proceeds from the side Hind Head and Leith Hill, to the coast of Sussex and the Isle of Wight, and principally affords the sides which will be hereafter used in finding the distance between Beachy Head and Dunnose. This branch also proceeds westward for the survey of the coast, and is bounded by the sides connecting the stations Leith Hill, Hind Head, Butser Hill, Dean Hill, and Wingreen on the north, and on the south by those connecting the stations Nine Barrow Down, Motteston Down, Dunnose, Rook's Hill, Chanctonbury Ring, and Ditchling Beacon.

The third branch, is that which proceeds from the side Hanger Hill and Banstead, to Botley Hill and Leith Hill, and from thence towards Beachy Head and Brightling, joining the series formerly projected at Botley Hill and Fairlight Down; the branch being bounded to the westward by the sides connecting the stations Hanger Hill, Banstead, Leith Hill, Ditchling Beacon, and Beachy Head.

The fourth branch, or remaining class of triangles, is that by which the distance between Beachy Head and Dunnose is obtained, and is formed by the sides connecting the stations Beachy Head, Ditchling Beacon, Chanctonbury Ring, Rook's Hill, and Dunnose.

ART. II. *Of the Selection of the Angles constituting the principal Triangles, and the Manner of reducing them for Computation.*

The angles of the several triangles, constituting the general series, are, with a very few exceptions, those arising from using the means of the several observations given in the foregoing part of this work; for although the rejecting of such as might apparently suit the purpose, would give the sums of the three angles of many of the triangles, nearer to 180 degrees *plus* the computed excess; yet as all the observations have been made with equal care, and are for the most part to be considered as of equal accuracy, it has been thought proper to select those means, as being the fairest mode of proceeding.

If the observations had been made on a sphere of known magnitude, and the angles accurately taken, the most natural method of computing the sides of the triangles from the measured bases, would be by spherical trigonometry; but if the magnitude was such, that the length of a degree of a great circle was equal to a degree of the meridian in these latitudes nearly, in order to obtain the sides true to a foot from such computation, with any facility, a table of the logarithmic sines of small arcs computed to every $\frac{1}{100}$ of a second of a degree, would be necessary, because the length of a second of a degree on the meridian is about 100 feet. As the lengths of small arcs and their chords are nearly the same (the difference in these between Beachy Head and Dunnose being less than 4 feet) it is evident this business might be performed sufficiently near the truth in any extent of a series of triangles, by plane

trigonometry, if the angles formed by the chords could be determined pretty exact. We have endeavoured to adopt this method in computing the sides of the principal triangles, in order to avoid an arbitrary correction of the observed angles, as well as that of reducing the whole extent of the triangles to a flat, which evidently would introduce erroneous results, and these in proportion as the series of triangles extended.

The length of a degree on the meridian in these latitudes being about 60874 fathoms, and that of a degree perpendicular to the meridian, about 61183; it follows, that the values of all the oblique arcs are between these extremes: now having obtained the sides of the triangles within a few feet by a rough computation, we take their values in parts of a degree, nearly as their inclinations to the meridian; this proportion, though not found on an ellipsoid, is sufficiently true for finding the values of the sides of the triangles; for in this case great accuracy is not necessary. With the sides thus determined, we compute the three angles of each triangle by spherical trigonometry; and taking twice the natural sines of half the arcs, we get, by plane trigonometry, the angles formed by the chords; then, from the differences of these angles we infer the corrections to be applied to the observed angles, to reduce them for computation: an example, however, will make this matter much plainer; for which purpose we shall take the very oblique triangle formed by the stations Beachy Head, Chanctonbury Ring, and Rook's Hill.

Arc between	Rook's Hill and B. Head	39′ 7″	chords	113785156
	Ch. Ring and B. Head	25 47		75000501
	Rook's Hill and Ch. Ring	14 0		40724320

Hence the angles by spherical trigonometry will be

	°	′	″
At Chanctonbury Ring - -	157	59	36,29
Rook's Hill - - - -	14	17	58,32
Beachy Head - - -	7	42	26,56
And the angles formed by the chords -	157	59	27,44
	14	18	3,44
	7	42	29,12

The respective differences are in the fourth column (triang. XXXIX.) In like manner the other differences in the same column have been obtained.

We have given the results to the second place in decimals, though perhaps they are true only to the nearest $\frac{1}{10}$ of a second.

In finding the angles formed by the chords, we have used RHETICUS's large *Triangular Canon*, where the natural sines are given to every 10″ of the quadrant, and computed to the radius 10000000000.

It is remarked, that great accuracy in the values of the sides in the degrees, &c. is not necessary, and that this is true will be found on examination; for in the foregoing example, if the sides of the triangle be varied, so that the resulting angles are several minutes different from those found above, still the differences between the spherical and plane triangles will be very nearly the same.

When the three angles of any triangle appear to have been observed correctly, by their sum being equal to 180 degrees *plus* the computed excess, the corrections for the chord angles have been added to, or taken from them, as that correction has been negative or affirmative, and the triangle rendered fit for computation. Also, if in any triangle, where the sum has either fallen short of, or exceeded 180 degrees *plus* the com-

puted excess, one or two of the observed angles have appeared to have been determined with sufficient accuracy, as shown by the agreement of the angles obtained upon different parts of the arch; the corrections for the chord angles have been added to, or taken from them, and the remaining angle or angles considered as erroneous. In the case of one angle being supposed right, and the other two wrong, the errors have been considered equal between the latter, unless the sum of the angles round the horizon at one of the stations, has indicated, that either the whole, or the greatest part of the excess or defect, was due to a particular angle. Likewise, when any triangle has been found in excess or defect, and all the angles have appeared to be determined with equal accuracy, the corrections for the reduction to the angles formed by the chords have been first applied, and then the errors considered equal.

What is called the spherical excess in the fifth column, is computed according to the rule, page 171. Phil. Transac. Vol. LXXX. These excesses above 180° would, of course, be exactly the same as the respective sums of the differences in the fourth column, if both were not obtained from approximating rules.

It is almost unnecessary to remark, that no computations have been attempted with the chords of the sides of the lesser triangles in the principal series.

ART. III. BRANCH I. Consisting of the Triangles which connect the Base of Departure on Hounslow Heath with that of Verification on Salisbury Plain, being bounded by the Sides connecting the Stations, Hanger Hill, St. Ann's Hill, Bagshot Heath, Highclere, Beacon Hill, and Four Mile-stone on the North; and on the South Side, by those connecting the Stations Dean Hill, Butser Hill, Hind Head, Leith Hill, and Banstead.

Distance from King's Arbour to Hampton Poor House, 27404,2 Feet.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″		″	″	° ′ ″	Feet.
I.	St. Ann's Hill -	44 18 52,25				44 18 51,75	
	Hampton Poor House	61 26 34,5				61 26 33,75	
	King's Arbour -	74 14 35,25				74 14 34,5	
		180 0 2		0,21	+ 1,79		
	St. Ann's Hill from Hampton Poor House -						37753,5
	St. Ann's Hill from King's Arbour - -						34455,2
II.	Banstead -	25 15 42,25				25 15 41	
	King's Arbour -	71 46 23,25				71 46 22	
	St. Ann's Hill	82 57 58,25				82 57 57	
		180 0 3,75		0,62	+ 3,13		
	Banstead King's Arbour -						80131,6
	Banstead St. Ann's Hill -						76667,7
III.	Hanger Hill -	24 39 16,5				24 39 16,5	
	Hampton Poor House	130 3 3,25				130 3 3,	
	St. Ann's Hill	25 17 40,75				25 17 40,5	
		180 0 0,5		0,26	+ 0,24		
	Hanger Hill Hampton Poor House -						38670,0
	Hanger Hill St. Ann's Hill -						09278,3

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
IV.	Banstead -	53 22 39,75	− 0,35			53 22 39,5	
	Hanger Hill -	62 40 34,75	− 0,39			62 40 34,25	
	St. Ann's Hill	63 56 46,75	− 0,39			63 56 46,25	
		180 0 1,25		1,1	+ 0,15		
	Banstead { Hanger Hill - -						77547,4 *
	Banstead { St. Ann's Hill -						76688,4

By these triangles, the distances from St. Ann's Hill to Banstead are 76687,7 feet, and 76688,4 feet; the mean of which is 76688 feet; and with this distance the sides marked with asterisks have been determined by working back.

Banstead from St. Ann's Hill, 76688,0 feet.

V.	Leith Hill -	58 19 22,5	− 0,35			58 19 22,25	
	Banstead -	77 37 35,5	− 0,44			77 37 35,	
	St. Ann's Hill	44 3 3	− 0,33			44 3 2,75	
		180 0 1		1,1	− 0,1		
	Leith Hill { Banstead - -						62655,2
	Leith Hill { St. Ann's Hill -						88019,8

Quadrilateral, formed by the Sides, St. Ann's Hill and Bagshot Heath, Bagshot Heath and Hind Head, Hind Head and Leith Hill, Leith Hill and St. Ann's Hill.

St. Ann's Hill from Leith Hill 88019,8 Feet.

VI.	Hind Head -	51 10 39,75	− 0,5			51 10 39,25	
	St. Ann's Hill	46 40 30,5	− 0,47			46 40 30,25	
	Leith Hill -	82 8 51	− 0,7			82 8 50,5	
		180 0 1,25		1,7	− 0,45		
	Hind Head { St. Ann's Hill -						111917,4 *
	Hind Head { Leith Hill -						82187,8 *

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
VII.	Bagshot Heath	47 57 7	− 0,53			47 57 6,5	
	Leith Hill -	56 37 29,5	− 0,53			56 37 29	
	Hind Head	75 25 25,25	− 0,63			75 25 24,5	
		180 0 1,75		1,7	+ 0,05		
	Bagshot Heath { Leith Hill - -						107115,9 •
	Bagshot Heath { Hind Head -						92425,9 •
VIII.	Bagshot Heath	53 52 14,25	− 0,16			53 52 14,25	
	Leith Hill -	25 31 21,5	− 0,2			25 31 22	
	St. Ann's Hill	100 36 23,5	− 0,6			100 36 23,75	
		179 59 59,25		0,96	− 1,71		
	Bagshot Heath from St. Ann's Hill -						46955,3 •
IX.	Bagshot Heath	101 49 22,25	− 0,62			101 49 21,75	
	Hind Head -	24 14 45,5	− 0,21			24 14 45,25	
	St. Ann's Hill	53 55 53	− 0,17			53 55 53	
		180 0 0,75		1,0	− 0,25		
	Bagshot Heath from St. Ann's Hill -						46955,4 •

Bagshot Heath from Hind Head 92425,9 Feet.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
X.	Highclere -	34 46 15,75	− 0,81			34 46 15	
	Bagshot Heath	83 20 14,25	− 1,36			83 20 14	
	Hind Head -	34 46 15,75	− 0,88			61 53 31	
		180 0 1,75		3,09	− 1,34		
	Highclere { Bagshot Heath -						142952,6 •
	Highclere { Hind Head - -						160972,2 •

No. of triangles	Names of stations.	Observed angles.	Diff.	Spheri-cal excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XI.	Butser Hill -	84 31 45,5	— 1,2			84 31 44,5	
	Hind Head	66 15 54,5	— 0,83			66 15 54,25	
	Highclere -	29 12 22	— 0,72			29 12 21,25	
		180 0 2		2,7	— 0,7		
	Butser Hill { Hind Head - -						78905,7*
	Butser Hill { Highclere -						148031,0*
XII.	Dean Hill -	62 22 48,75	— 1,37			62 22 47	
	Butser Hill	48 28 41,5	— 1,23			48 28 40	
	Highclere -	69 8 35	— 1,5			69 8 33	
		180 0 5,25		4,07	+ 1,18		
	Dean Hill { Butser Hill - -						156122,1*
	Dean Hill { Highclere -						125084,9*
XIII.	Beacon Hill -	102 45 23,5	— 0,9			102 45 22	
	Highclere -	26 55 51,5	— 0,26			26 55 50,75	
	Dean Hill	50 18 47,5	— 0,15			50 18 47,25	
		180 0 2,5		1,3	+ 1,2		
	Beacon Hill { Highclere - -						98694,4
	Beacon Hill { Dean Hill -						58086,3

Triangles which connect the Base of Verification with the Sides Beacon Hill and Highclere, and Beacon Hill and Dean Hill.

XIV.	Thorney Down	53 22 30				53 22 31,25	
	Highclere -	12 59 10				12 59 10,75	
	Beacon Hill	113 38 16,75				113 38 18	
		179 59 56,75	-	0,6	— 3,85		
	Thorney Down { Highclere - -						112656
	Thorney Down { Beacon Hill -						27634,4

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XV.	Old Sarum -	48 26 4,5				48 26 4,5	
	Thorney Down	98 0 31				98 0 30,75	
	Beacon Hill -	33 33 24,75				33 33 24,75	
		180 0 0,25		0,13	+ 0,12		
		Old Sarum from Thorney Down -					20416,1
XVI.	Four Mile-stone	72 4 48				72 4 47,5	
	Dean Hill -	39 29 3,25				39 29 3	
	Beacon Hill -	68 26 10				68 26 9,5	
		180 0 1,25		0,5	+ 0,75		
		Four Mile-stone { Dean Hill - -					56775,0
		Four Mile-stone { Beacon Hill -					38818,2
XVII.	Old Sarum -	85 58 22,5				85 58 21,75	
	Four Mile-stone	70 1 47,5				70 1 47	
	Beacon Hill -	23 59 51,75				23 59 51,25	
		180 0 1,75		0,14	+ 1,61		
		Old Sarum from Four Mile-stone -					15826,4

ART. IV. *The Length of the Base of* VERIFICATION *deduced from that on* Hounslow Heath, *and the foregoing Triangles.*

The base on Hounslow Heath is 27404,2 feet, which, with the four first triangles, give 76688 feet for the mean distance of St. Ann's Hill and Banstead.

That mean distance, with the 5, 6, 7, 10, 11, 12, 13, 16, and 17th triangles, will give 36574,7 feet for the base of *verification.*

If the computation be made with the 8 and 9th triangles also, and the mean distance taken between Hind Head and Bagshot, the base will be 36574,3.

And those mean distances of St. Ann's Hill and Banstead, and Hind Head and Bagshot, with the 14 and 15th triangles (excluding the 16 and 17th), will produce 36574,6, and 36574,9 respectively.

Lastly;—if the computations are carried directly from one base to the other, independent of the mean distances and the 14 and 15th triangles, the greatest and least results will be 36574,8, and 36573,8, the mean being 36574,3 feet, or about an inch short of the measurement.

Of the several ways by which the base of verification, or distance between Beacon Hill and Old Sarum is deduced, the first seems to have the preference, because the angles of the 6 and 7th triangles appear to have been observed very correctly. The results from the 14 and 15th triangles cannot be considered as very conclusive, because the angle at Highclere is so acute that a trifling error in it will vary the distance from Beacon Hill to Thorney Down very considerably: and we had some reasons for being dissatisfied with this angle, and also that in the same triangle at Thorney Down, on account of the strain in the clamp. See Sect. II. Art. VI.

Although the result of this comparison might afford some reason for supposing, that the sides of the triangles in this branch would be sufficiently near the truth, were all of them computed from the base on Hounslow Heath, yet, to approach more nearly to their correct distances, those which are marked with asterisks, have been computed with each base, and a mean of the results taken. The remaining sides have been determined by the bases in their vicinity.

ART. V. BRANCH II. Consisting of the Triangles which are bounded by the Sides connecting the Stations Leith Hill, Hind Head, Butser Hill, Dean Hill, Beacon Hill, Wingreen, Nine Barrow Down, Motteston Down, Dunnose, Rook's Hill, Chanctonbury Ring, and Ditchling Beacon.

Hind Head from Leith Hill 82187,8 Feet, mean Distance.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XVIII.	Chanctonbury Ring	45 10 46,5	− 0,44			45 10 46	
	Leith Hill -	72 56 50,25	− 0,7			72 56 49,25	
	Hind Head -	61 52 25,5	− 0,62			61 52 24,75	
		180 0 2,25		1,8	+ 0,45		
	Chanctonbury Ring { Leith Hill -						102185,7
	Chanctonbury Ring { Hind Head -						110774,4
XIX.	Chanctonbury Ring	86 44 41	− 0,62			86 44 39,75	
	Leith Hill -	32 43 57,5	− 0,39			32 43 56,5	
	Ditchling Beacon	60 31 24,75	− 0,38			60 31 23,75	
		180 0 3,25		1,5	+ 1,75		
	Chanctonbury Ring from Ditchling Beacon						63469,1
XX.	Rook's Hill -	82 42 45,75	− 0,7			82 42 45,25	
	Chanctonbury Ring	47 12 38	− 0,45			47 12 38	
	Hind Head -	50 4 37	− 0,46			50 4 36,75	
		180 0 0,75		1,6	− 0,85		
	Rook's Hill from Chanctonbury Ring -						85645,4

Butser Hill and Hind Head. Branch I. 78905,7 Feet.

XXI.	Butser Hill -	70 25 13	− 0,39			70 25 13	
	Hind Head -	44 28 6,25	− 0,3			44 28 6,25	
	Rook's Hill -	65 6 40,75	− 0,36			65 6 40,75	
		180 0 0		1,1	− 1,1		
	Rook's Hill { Hind Head - -						81954,4
	Rook's Hill { Butser Hill -						60933,8

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XXII.	Dunnose -	24 44 15,5	— 0,52			24 44 16	
	Butser Hill -	80 21 58	— 0,81			80 21 58,5	
	Rook's Hill	74 53 45	— 0,65			74 53 45,5	
		179 59 58,5		1,96	— 3,46		
	Dunnose from Rook's Hill - -						143558,9
XXIII.	Dunnose -	55 43 7	— 1,53			55 43 6,75	
	Butser Hill -	76 12 22	— 1,99			76 12 21,5	
	Dean Hill -	48 4 32,25	— 1,54			48 4 31,75	
		180 0 1,25		5,0	— 3,75		
	Dunnose { Butser Hill -						140580,4
	Dunnose { Dean Hill - -						183496,2
XXIV.	Dunnose -	53 12 27,25	— 2,03			53 12 25,5	
	Dean Hill -	64 50 19	— 2,26			64 50 16,75	
	Nine Barrow Down	61 57 19,75	— 2,22			61 57 17,75	
		180 0 6		6,5	— 0,5		
	Dunnose from Nine Barrow Down - *						188181,8
	Distance from Beacon Hill to Dean Hill, as got by the Base on Salisbury Plain - - - - - - -						58086,3
XXV.	Wingreen -	30 13 23	— 0,35			30 13 22,5	
	Beacon Hill -	66 49 52,25	— 0,39			66 49 51,5	
	Dean Hill -	82 56 47	— 0,68			82 56 46	
		180 0 2,25		1,43	+ 0,82		
	Wingreen { Beacon Hill - -						114522,4
	Wingreen { Dean Hill - -						106089

* This distance is the mean, as derived from the Salisbury Base, and from the side Butser Hill and Dean Hill.

No of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XXVI.	Nine Barrow Down	39 34 28,75	– 0,82			39 34 28,25	
	Wingreen -	88 58 47,75	– 1,59			88 58 46,75	
	Dean Hill	51 26 45,5	– 0,82			51 26 45	
		180 0 2		3,24	– 1,24		
	Nine Barrow Down { Wingreen - -						130224,5
	Nine Barrow Down { Dean Hill -						166497
XXVII.	Motteston Down	—	– 1,71			72 48 37,5	
	Nine Barrow Down	56 9 55,25	– 1,43			56 9 53,75	
	Dean Hill -	51 1 30	– 1,3			51 1 28,75	
				4,41			
	Motteston Down { Nine Barrow Down -						135489,6
	Motteston Down { Dean Hill - -						144766
XXVIII.	Motteston Down	—	– 1,61			62 39 30,5	
	Dean Hill -	61 53 20,75	– 1,64			61 53 19	
	Butser Hill -	55 27 12	– 1,47			55 27 10,5	
				4,7			
	Motteston Down from Butser Hill -						155023,4
XXIX.	Motteston Down	64 41 2	– 0,35			64 41 4	
	Butser Hill	20 45 10	– 0,43			20 45 9,5	
	Dunnose -	94 33 47,5	– 1,0			94 33 46,5	
		179 59 59,5		1,8	– 2,3		
	Motteston Down from Dunnose - -						55104,3

The four sides of the first Branch, namely; Beacon Hill and Dean Hill, Dean Hill and Butser Hill, and Butser Hill and Hind Head, have been used in the computation of the sides of this branch, because they are supposed to be nearly true: had, however,

these triangles been considered as independent of those in the first branch, and the side Hind Head and Leith Hill been used as derived from the base on Hounslow Heath, nearly the same conclusions would have taken place; for the distance between Beacon Hill and Old Sarum would in that case be 36574,2 feet, which is only two and an half inches less than the measured base. This may be considered as a proof, that the angles of the triangles forming this branch are sufficiently correct, since the series which joins the two bases by this route, is nearly an hundred and twenty miles in extent. Some little variation in that result might be produced by a different correction of the angles of the 24th triangle: but as the angle at Butser Hill must be very nearly true, the other angles cannot, on any reasonable supposition, be so corrected as to make the computed base differ from the measured one more than six inches.

ART. VI. BRANCH III. Proceeding from the Side Hanger Hill and Banstead to Botley Hill and Leith Hill, and from thence to Brightling and Beachy Head, joining the Triangles with those of the late General ROY, at Botley Hill and Fairlight Down, being bounded to the westward by the Sides connecting the Stations Hanger Hill, Banstead, Leith Hill, Ditchling Beacon, and Beachy Head.

Hanger Hill from Banstead 77547,4 Feet.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XXX.	Shooter's Hill -	54 43 49,75				54 43 49,25	
	Hanger Hill -	62 18 50				62 18 49,5	
	Banstead -	62 57 22				62 57 21,25	
		180 0 1,75		1,4	+ 0,35		
	Shooter's Hill { Hanger Hill - -						84596,3
	Shooter's Hill { Banstead - -						84107

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XXXI.	Botley Hill -	85 39 58,5				85 39 58,25	
	Shooter's Hill	37 8 25,75				37 8 25,75	
	Banstead -	57 11 36				57 11 36	
		180 0 0,25		0,9	− 0,65		
	Botley Hill { Shooter's Hill -						70894,9
	Botley Hill { Banstead - -						50927
XXXII.	Leith Hill -	31 21 10	− 0,08			31 21 9,75	
	Banstead -	108 50 48,25	− 0,53			108 50 47,75	
	Botley Hill -	39 48 2,5	− 0,06			39 48 2,5	
		180 0 0,75		0,7	+ 0,05		
	Leith Hill from Botley Hill -						92631,5

In this triangle, using the side from Leith Hill to Banstead as got by the first branch, we find the distance between Leith Hill and Botley Hill to be 92632,9 feet; hence the mean distance is 92632,2 feet.

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
XXXIII.	Crowborough Beacon	46 12 11,75	− 0,45			46 12 11,25	
	Botley Hill -	89 35 1	− 0,98			89 35 0,25	
	Leith Hill -	44 12 49	− 0,45			44 12 48,5	
		180 0 1,75		1,9	− 0,15		
	Crowborough Beacon { Botley Hill - -						89492,5
	Crowborough Beacon { Leith Hill -						128331,9
XXXIV.	Ditchling Beacon	78 18 27	− 0,91			78 18 25,5	
	Crowborough Beacon	63 24 37,25	− 0,69			63 24 36,25	
	Leith Hill -	38 16 59,75	− 0,62			38 16 58,25	
		180 0 4		2,2	+ 1,8		
	Ditchling Beacon { Leith Hill - -						117190,4
	Ditchling Beacon { Crowborough Beacon -						81192,2

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XXXV.	Brightling -	43 29 1,5	– 0,16			43 29 1	
	Crowborough Beacon	105 2 44	– 0,76			105 2 42	
	Ditchling Beacon	31 28 17,75	– 0,22			31 28 17	
		180 0 3,25		1,14	+ 2,11		
	Brightling { Crowborough Beacon -						61597,6
	Brightling { Ditchling Beacon -						113942,3
XXXVI.	Beachy Head -	73 58 26,5	– 0,77			73 58 26,5	
	Ditchling Beacon	46 32 19	– 0,56			46 32 19,5	
	Brightling -	59 29 14	– 0,64			59 29 14	
		179 59 59,5		2,0	– 2,5		
	Beachy Head { Ditchling - -						102132,4
	Beachy Head { Brightling - -						86048
XXXVII.	Fairlight Down	59 33 1,75	– 0,39			59 33 1,75	
	Brightling -	80 44 19,25	– 0,51			80 44 19,25	
	Beachy Head -	39 42 39	– 0,36			39 42 39	
		180 0 0		1,28	– 1,28		
	Fairlight Down { Brightling - -						63773,1
	Fairlight Down { Beachy Head -						98513,7

ART. VII. *Comparison of the Distances from* Botley Hill *to* St. Ann's Hill, *and* Fairlight Down, *deduced from the recent Observations, and those of General* ROY *in* 1787, 1788.

The stations on St. Ann's Hill, Botley Hill, and Fairlight Down, connect our triangles with those of General ROY; and therefore the two distances from the middle station, Botley Hill, which are common to both series of triangles, afford

the readiest, and indeed almost the only means of comparing independent deductions from both operations; the triangle St. Ann's Hill, King's Arbour, Hampton Poor House excepted.

The distances from the station at the Hundred Acres to St. Ann's Hill and Botley Hill, according to General Roy (see the 4th and 9th triangles in his account) are 79211,22, and 48726,75 feet; and from the 4th, 5th, and 9th triangles it appears, that the included angle at that station is 169° 25′ 21″,25; these give 127424,3 feet for the distance of St. Ann's Hill and Botley Hill; this distance, however, is deduced from the base on Hounslow Heath, supposing it to be 27404,7 feet; but its mean length, according to both measurements, being 27404,2 feet, we shall have 27404,7 : 27404,2 : : 127424,3 : 127422 feet, for the distance of the stations from that mean length of the base.

According to our observations, the distances of St. Ann's Hill and Botley Hill from Leith Hill are 88019,8 and 92632,2 feet respectively, and the included angle for computation at Leith Hill 89° 40′ 32″; hence, from our triangles, the distance of the stations will be 127420 feet; which is 2 feet less than that from General Roy's triangles.

Before we compute the distance from Botley Hill to Fairlight Down, it will be necessary to premise, that an error has crept into General Roy's reduction of the measured base on Romney Marsh (see Phil. Trans. Vol. LXXX.); which, however, cannot be discovered without consulting his account of the measurement of the other base on Hounslow Heath. We are informed (page 131, Vol. LXXX.), that when the new points on the chain were laid off from the original points on the great plank in Mr. Ramsden's shop, Fahrenheit's ther-

mometer was at $55°$*, but the temperature is omitted *when those points in the plank were transferred from the brass standard.* The "original points" must be those alluded to in the General's account of the Hounslow Heath base (Phil. Trans. Vol. LXXV. p. 403), which were fixed in the plank from the brass standard in the temperature of $63°$; but it is probable that General Roy supposed them to have been transferred in $62°$, and, through mistake, subtracted the sum of the two first corrections in page 131, instead of their difference, which in that case would have been the true correction for the contraction of the chain. The error however, is about 33 inches: for since the chain in the temperature of $55°$ was equal to 100 feet of the brass standard in that of $63°$, it follows, from the table of expansions in the General's account of the Hounslow Heath base, that its length in $53°\frac{4}{10}$ was equal to 100 feet of the brass standard in $62°$; and therefore $53°\frac{4}{10}$ is the temperature to which the measurement by the chain should be reduced. Now the apparent length being 258,36736 chains, and 68290,5 the sum of all the degrees shown by the thermometers in the table, page 134, we have $\overline{285{,}36736 \times 53\frac{4}{10} - \frac{68290{,}5}{5}} \times {,}00763$ inches $= 12{,}8$ inches, the contraction below $53°\frac{4}{10}$; this, with the other corrections applied to the apparent length, give 28535 feet 8 inches, instead of 28532 feet 11 inches.

To determine the distance from Hollingbourn Hill to Fairlight Down from this base (28535,66 feet) by means of the fewest triangles, we suppose, according to General Roy (page

* That this was the temperature, appears in a great degree from various comparisons we made with the chain and the two new ones on Hounslow Heath: Sir J. Banks very obligingly favoured us with the Society's chain, for the purpose of trying its length with the new chains.

177) that the observed angle at Hollingbourn Hill, between Allington Knoll and Fairlight Down, was 48° 56′ 31″,5, and reduce it to 48° 56′ 30″ for computation; then from the 24th, 23d, and 22d triangles, and the triangle

Hollingbourn Hill	-	48°	56′	30″
Allington Knoll	-	88	25	44
Fairlight Down		42	37	46

we get 141759,6 feet for the distance of Hollingbourn Hill and Fairlight Down.

The distance of those stations as deduced from the other base (27404,7) is 141748,5 (see remarks in Vol. LXXX. p. 595); hence 27404,7 : 27404,2 : : 141748,5 : 141746 feet nearly, their distance from the mean of the measurements on Hounslow Heath; therefore the mean distance resulting from both bases is 141753 feet nearly. Now with this distance, and the 13th, 12th, and 11th triangles, we shall find the distance from Hollingbourn Hill to Botley Hill 150971 feet; and the angle at Hollingbourn Hill, between Botley Hill and Fairlight Down 88° 27′ 0″,25; these will give the distance from Botley Hill to Fairlight Down, 204275,5 feet.

To determine this line from our triangles, we have 92632,2 and 117190,4 feet for the distances of Botley Hill and Ditchling Beacon from Leith Hill; also 102132,4 and 98513,7 feet for the distances of Ditchling Beacon and Fairlight Down from Beachy Head; from these, with the included angles at Leith Hill and Beachy Head, we find Ditchling Beacon from Botley Hill 139567,4, and from Fairlight Down 167986,5 feet, and the included angle at Ditchling Beacon 82° 41′ 6″,8; hence the distance from Botley Hill to Fairlight Down will be 204276 feet nearly.

So near an agreement in a length of almost 39 miles, can only be attributed to chance.

Hence it appears, that a difference of 5 or 6 feet in about 27 miles (the distance of the stations Hollingbourn Hill and Fairlight Down), may be supposed in General ROY's deductions on account of the variations, or corrections in the bases on Hounslow Heath, and Romney Marsh; this difference, however, is too trifling to be of consequence in any of his principal conclusions.

ART. VIII. BRANCH IV. *Consisting of the nearest Triangles to the northward of* Beachy Head *and* Dunnose, *for finding the Distance between those Stations.*

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.
		° ′ ″	″	″	″	° ′ ″
XXXVIII.	Dunnose -	15 43 0	+ 0,55			15 43 0,5
	Rook's Hill -	137 16 48,5	− 3,88			137 16 44,5
	Chanctonbury Ring	27 0 13	+ 1,37			27 0 15
		180 0 1,5		1,96	− 0,46	

By this triangle, using the distance from Rook's Hill to Chanctonbury Ring as found by the first branch, we get the distance between Rook's Hill and Dunnose, 143559,3 feet; but by the same branch, 143558,9 feet was found to be the distance; and if the side Butser Hill and Dean Hill be made the base, we shall get, by the 22d and 23d triangles, the distance from Rook's Hill to Dunnose 143557,1 feet: hence 143558,4, the mean of these three distances with the above triangle, give 214498,4 feet, for the distance between Dunnose and Chanctonbury Ring.

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.
		° ′ ″	″	″	″	° ′ ″
XXXIX.	Beachy Head -	7 42 37	+ 2,56			7 42 40
	Rook's Hill -	14 17 33,25	+ 5,12			14 17 38
	Chanctonbury Ring	157 59 50,75	− 8,85			157 59 42
		180 0 1		1,19	− 0,19	

By this triangle, with the side Chanctonbury Ring and Rook's Hill, as found by the second branch, we get the distance between Chanctonbury Ring and Beachy Head, 157592,5 feet; and by the following triangle

XL.	Beachy Head -	13 58 29,5	+ 0,48			13 58 28
	Ditchling Beacon -	143 9 31,5	− 2,35			143 9 30
	Chanctonbury Ring	22 52 3,25	+ 0,99			22 52 2
		180 0 4,25		0,9	+ 3,35	

using the side Chanctonbury Ring and Ditchling Beacon as got by the second branch, we get another distance between Beachy Head and Chanctonbury Ring, namely, 157590,8 feet; wherefore the mean distance is 157591,6; and this, with the 39th triangle, give 239160,2 feet for the distance between Rook's Hill and Beachy Head: hence we have four principal distances, namely,

$$\text{Dunnose from}\left\{\begin{array}{ll}\text{Rook's Hill} \quad - & 143558{,}4\\ \text{Chanctonbury Ring} & 214498{,}4\end{array}\right\}\text{feet.}$$

$$\text{Beachy Head from}\left\{\begin{array}{ll}\text{Rook's Hill} \quad - & 239160{,}2\\ \text{Chanctonbury Ring} & 157591{,}6\end{array}\right\}\text{feet.}$$

And these sides used in the following triangles,

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.
		° ′ ″	″	″	″	° ′ ″
XLI.	Beachy Head -	20 46 53	— 0,2			20 46 52,75
	Rook's Hill -	122 59 14,5	— 7,7			122 59 8
	Dunnose -	36 13 58	+ 1,17			36 13 59,25
		180 0 5,5		6,77	— 1,27	
XLII.	Dunnose -	20 30 58	+ 0,86			20 30 58,75
	Chanctonbury Ring	130 59 37,75	— 8,77			130 59 29
	Beachy Head -	28 29 30	+ 1,92			28 29 32,25
		180 0 5,75		6,01	— 0,26	

give the four distances of Beachy Head from Dunnose, as beneath;

$$\left.\begin{array}{l}339394{,}6\\339395{,}0\\339399{,}2\\339401{,}5\end{array}\right\}\text{feet.}$$

Hence 339397,6, the mean, may be considered as very nearly the true distance.

In the correction of the angles of the triangles which compose this branch, we have been a little more particular than with the others of the series, as it is of much consequence that the distance between Beachy Head and Dunnose should not be left doubtful.

In the 42d triangle, it must be observed, that there is a defect of $\frac{1}{4}''$ nearly in the sum of the observed angles; in the 38th, about $\frac{1}{2}$ a second; and in the 41st, a defect of about $1''\frac{1}{4}$: the sum in the 39th is nearly right, but the angles of it are considered as residuary, or remaining angles; the triangle being too oblique to be admitted as a principal one in the series, though numbered and inserted as such.

Now it is evident, that if all the angles of the four triangles contained in the quadrilateral formed by the stations on Dunnose, Rook's Hill, Chanctonbury Ring, and Beachy Head, were accurately corrected for computation, the distance from Beachy Head to Dunnose would be found the same from each triangle, by making use of the side Rook's Hill and Chanctonbury Ring (which is common to the two most oblique ones): therefore, having assumed that distance, we found by computation, that if each of the above errors is supposed to be in *one angle* only of the respective triangles, these angles must be the three observed ones, namely, 28° 29′ 30″; 27° 0′ 13″; and 122° 59′ 14″,5; these are augmented accordingly, before the angles are finally corrected for computation. The angles of the 39th triangle, resulting from those of the other triangles, are

Chanctonbury Ring -	157°	59′	51″,25
Rook's Hill - -	14	17	32,75
Beachy Head -	7	42	37,25

before they are reduced to the angles formed by the chords.

ART. IX. *Containing the Triangles belonging to the Series which have had only two of their three Angles observed.*

Highclere and Beacon Hill 98694,4 feet.

Names of stations.	Observed angles.	Spherical excess.	Angles corrected for calculation.	Distances.
	° ′ ″		° ′ ″	Feet.
Inkpin Beacon -			106 26 52,25	
Highclere -	56 0 29,75		56 0 29,5	
Beacon Hill -	17 32 38,5		17 32 38,25	
Inkpin Beacon { Highclere				30948
Inkpin Beacon { Beacon Hill				85321

Wingreen and Beacon Hill 114522,4 feet.

Names of stations.	Observed angles.	Spherical excess.	Angles corrected for calculation.	Distances.
Long Knoll -			57 50 39,75	
Beacon Hill -	31 11 43,25		32 11 43	
Wingreen -	89 57 37,75		89 57 37,25	
Long Knoll { Beacon Hill -				135272
Long Knoll { Wingreen -				72074

Wingreen and Nine Barrow Down 130224,5 feet.

Names of stations.	Observed angles.	Spherical excess.	Angles corrected for calculation.	Distances.
Bull Barrow -			93 33 0,75	
Nine Barrow Down	31 57 25,25		31 57 25	
Wingreen -	54 29 25,75		54 29 34,25	
Bull Barrow { Nine Barrow Down				106212,2
Bull Barrow { Wingreen -				69058

Names of stations.	Observed angles.	Spherical excess.	Angles corrected for calculation.	Distances.
	° ′ ″		° ′ ″	Feet.
Bull Barrow -			84 31 24	
Nine Barrow Down	38 58 19,25		38 58 18,75	
Black Down -	56 30 18,5		56 30 17,25	
Black Down { Nine Barrow Down				126781,2
Black Down { Bull Barrow				80103,6

With respect to this last triangle, it must be observed, that in the future prosecution of the survey, the side Bull Barrow and Blackdown will be obtained by another method, the result of which, when combined with that given by this triangle, will afford a more accurate means of determining other distances which will hereafter depend upon it. This triangle, and likewise the rest of them in this article, are inserted here, as the distances deduced from them are supposed to be nearly true; they may possibly be of some service at present; but at a future period they will be given in a more perfect state.

ART. X. *Triangles for finding the Distance between Nettlebed and Shooter's Hill.*

Shooter's Hill from Botley Hill, 70894,9 feet.

Names of stations.	Observed angles.	Spherical excess.	Angles corrected for calculation.	Distances.
	° ′ ″	″	° ′ ″	Feet.
Leith Hill -	23 20 51		23 20 51	
Botley Hill -	125 28 1		125 28 1,25	
Shooter's Hill -	31 11 7,5		31 11 7,75	
	179 59 59,5	1,23		
Leith Hill from Shooter's Hill -				145696,2

St. Ann's Hill and Leith Hill, 88019,8 feet.

Shooter's Hill -	36 8 50,75		36 8 49,5	
St. Ann's Hill -	77 31 32,75		77 31 30,75	
Leith Hill -	66 19 41,5		66 19 39,75	
	180 0 5	2,77		
Shooter's Hill { St. Ann's Hill -				136665,5
Shooter's Hill { Leith Hill -				145698,6

Hence the mean distance between Shooter's Hill and Leith Hill is 145697,4.

Hind Head and Leith Hill, 82187,8 feet.

Names of stations.	Observed angles.	Spherical excess.	Angles corrected for calculation.	Distances.
	° ′ ″	″	° ′ ″	Feet.
Nettlebed -			23 44 58,75	
Hind Head -	94 9 57,5		94 9 56,25	
Leith Hill -	62 5 6		62 5 5	
	180 0 5	3,48		
Nettlebed { Hind Head - -				180325,4
Nettlebed { Leith Hill - -				203531,5

Then by using the sides Shooter's Hill and Leith Hill, and Nettlebed and Leith Hill, in the following triangle,

Shooter's Hill	56 48 31		56 48 29	
Leith Hill	86 23 25,75		86 23 23,25	
Nettlebed			36 48 7,75	
		6,97		

we get 242730 and 242732 feet for the distance of Shooter's Hill from Nettlebed, the mean being 242731 feet.

SECTION V.

Of the Direction of the Meridians at Dunnose and Beachy Head; and the Length of a Degree of a great Circle, perpendicular to the Meridian, in Latitude 50° 41′.

ART. I. *Of the Direction of the Meridian at Dunnose with respect to Brading Staff.*

	°	′	″
On April 28th in the afternoon, the angle between the pole star, when at its greatest apparent elongation from the meridian, and the staff, was observed - - - - -	24	4	23
And on April 29th in the morning -	18	24	0
Wherefore half their sum is the angle between the meridian and Brading staff, namely -	21	14	11,5
On May 12th, in the afternoon, the angle between the star and staff was observed -	24	4	29,5
And on May 13th, in the morning -	18	23	53,25
Wherefore half their sum is the angle between the meridian and Brading staff, namely -	21	14	11,4

Hence 21° 14′ 11″,5, may be taken for the angle between the meridian and Brading staff, as determined by the double azimuths.

The apparent polar distances of the star, on those days which do not refer to corresponding observations on the opposite side of the meridian, are as follow:

	°	′	″		Azim. °	′	″
April 21st	1	47	57,2	which, with the lat. of Dunnose, *viz.* 50° 37′ 8″ nearly, give the azimuths for those days	2	50	11,2
April 22d	1	47	57,4		2	50	11,5
May 5th	1	48	0,7		2	50	16,8

	°	′	″
And these subtracted from the observed angles (see Sect. II. Art. VII.) give -	21	14	10,05
	21	14	10,5
	21	14	10,45

The mean of which is 21° 14′ 10″,3 for the angle between the meridian and the staff, which is a little more than 1″ different from that obtained by the double azimuths; we shall, however, take 21° 14′ 11″,5 for the true angle.

ART. II. *Of the Direction of the Meridian at* Beachy Head *with respect to* Jevington *Staff.*

	°	′	″
On August 1st, in the morning, the angle between the pole star and the staff was observed	24	38	20,25
And at night - - - -	30	19	49,5
Therefore half their sum is the angle between the meridian and Jevington staff, namely -	27	29	5
On August 2d, at night, the angle between the star and staff was observed - -	30	19	50,25
And on August 3d, in the morning -	24	38	23,5
Therefore half their sum is the angle between the meridian and Jevington staff, namely -	27	29	7

Hence 27° 29′ 6″, the mean by the double azimuths, may be taken as the angle between the meridian and the staff.

The apparent polar distances of the star, on those days which do not refer to corresponding observations on the opposite side of the meridian, are as follow:

		°	′	″		Azim. °	′	″
July	15th	1	48	4,6	which, with the latitude of Beachy Head, viz. 50° 44′ 25″ nearly, give the azimuths for those days -	2	50	49,4
	16th	1	48	4,4		2	50	49,1
	26th	1	48	2,9		2	50	46,7
	30th	1	48	2		2	50	45,3
Aug.	11th	1	47	59,3		2	50	41

		°	′	″
And these applied to the observed angles, give -	{	27	29	5,1
		27	29	8,4
		27	29	5,7
		27	29	5,2
		27	29	6,25

The mean of which is 27° 29′ 6″,1, for the angle between the meridian and Jevington staff, being the same as that obtained from a mean of the double azimuths.

ART. III. *Determination of the Length of a Degree of a great Circle, perpendicular to the Meridian, in Latitude* 50° 41′.

In Tab. XLV. fig. 1. let D and B be Dunnose and Beachy Head, and P the pole, forming the spheroidical triangle D P B; and let C and A be the staffs at Jevington and Brading Down, respectively.

	°	′	″
Now the angle at Dunnose, between the meridian and the staff, or P D A, was found by the double azimuths to be - - - -	21	14	11,5
And the angle between the staff and the station on Beachy Head, or A D B - -	60	42	41,5
Therefore their sum is the angle between the meridian and the station on Beachy Head, or P D B; which is - - -	81	56	53
Again; at Beachy Head the angle between the meridian and the staff, or P B C, was found by the double azimuths to be - - -	27	29	6
And the angle between the staff and the station on Dunnose, or C B D - -	69	26	52
Therefore their sum is the angle between the meridian and the station on Dunnose, namely -	96	55	58

Hence, in the spheroidical triangle DPB, we have the angles P D B and P B D given.

Again. in fig. 2. let P G M be the meridian of Greenwich; then, if M B be the parallel to the perpendicular at G, Greenwich, we shall get (by Sect. VI. Art. II.) M B = 58848 feet, and G M = 269328 feet; therefore, taking 60851 fathoms for the length of the degree on the meridian, as derived from the difference of latitude between Greenwich and Paris, applied to the measured arc (see Phil. Trans. Vol. LXXX.) we get G M = 44′ 15″,26; consequently the latitude of the point M, (that of Greenwich being 51° 28′ 40″), is 50° 44′ 24″,74; and the co-lat. P M = 39° 15′ 35″,26.

With respect to the value of the arc M B, for the present purpose, it is not of consequence on what hypothesis it be obtained; but if 61173 fathoms be assumed for the length of a degree of a great circle perpendicular to the meridian at M, then M B = 9′ 37″,19, and the latitude of B, or Beachy Head, will be found = 50° 44′ 23″,71.

Again; in fig. 3. let WB be the arc of a great circle perpendicular to the meridian of Beachy Head at B, meeting that of Dunnose in W; and let D R be another arc of a great circle perpendicular to the meridian of Dunnose in D, meeting that of Beachy Head in R; then we shall have two small spheroidical triangles W B D and R D B having in each two angles given, namely, W D B = 81° 56′ 53″, and W B D = 6° 55′ 58″ in the triangle W B D; and D B R = 83° 4′ 2″, with B D R = 8° 3′ 7″ in the triangle D B R; and these reduced to the angles formed by the chords, give the following triangles for computation, namely,

In the triangle W B D $\begin{cases} WBD = 6^\circ\ 55'\ 57'',2 \\ WDB = 81\ 56\ 52,4 \\ DWB = 91\ \ 7\ 10,4 \end{cases}$

And in the triangle B D R $\begin{cases} BDR = 8\ \ 3\ \ 6 \\ DBR = 83\ \ 4\ \ 1 \\ DRB = 88\ 52\ 53 \end{cases}$

In which it must be noted, that the reduced angles are given to the nearest $\frac{1}{4}''$.

Now the chord of the arc B D, or the distance between Beachy Head and Dunnose, is 339397,6 feet (vide Sect. IV. Art. VIII.), which used in the

Triangle W B D gives - $\begin{cases} BW = 336115{,}6 \text{ feet} \\ DW = 40973{,}4 \text{ feet} \end{cases}$ and the triangle B D R - $\begin{cases} DR = 336980 \text{ feet} \\ BR = 47547{,}1 \text{ feet.} \end{cases}$

Again; let B L and D E be the parallels of latitude of Beachy Head and Dunnose, meeting the meridians in L and E: then, to find LW and E R we have two small triangles which may be considered as plane ones, namely, L B W and E D R, in which the angles at W and R are given, nearly.

Now the excess of the three angles above 180° in the triangle DBW, considered as a spherical one, is 3″ nearly; therefore the angle DWB will be 91° 7′ 12″ nearly; hence BWL = 88° 52′ 48″: consequently the angle BLW = 90° 33′ 36″, and LBW = 0° 33′ 36″. Therefore with the chord of the arc WB = 336115,6 feet, we get W L = 3285,2 feet, which added to W D, as found above, gives 44258,6 feet, for the distance between the parallels of Beachy Head and Dunnose.

Again; in the triangle B D R, considered as a spherical one, the excess is about $3''\frac{1}{2}$; hence, from the two observed angles at D and B, namely, 8° 3′ 7″, and 83° 4′ 2″, we get the third angle BRD = 88° 52′ 54″,5; and taking the triangle ERD as a plane one, the other angles will be 0° 33′ 32″,75 (EDR), and 90° 33′ 32″,75 (DER); therefore, with the chord

of the arc DR = 336980 feet, we get RE = 3288,2 feet, which taken from BR, as found above, leaves 44258,9 feet for the meridional arc, or the distance between the parallels of Beachy Head and Dunnose; which is nearly the same as before.

This method of determining the distance between the parallels is sufficiently correct; but the same conclusion may be deduced from a different principle, thus:

Let the difference of longitude, or the angle at P, be found, on any hypothesis of the earth's figure, and likewise the latitudes of Beachy Head and Dunnose; with these compute the latitudes of the points E and L; then it will be found, that the arc RE is $\frac{5}{100}''$ greater than LW; and since $\frac{1}{100}$ of a second on the meridian is nearly a foot, RE is 5 feet more than LW; hence $\frac{47547,1 - 5 + 40973,4}{2} = 44257,8$ feet is the distance between the parallels, and which is very nearly the same as found by the other method.

It seems therefore, that whatever be the value of the arch between those parallels in parts of a degree, the distance between them is obtained sufficiently near the truth; therefore, taking 60851 fathoms for the length of a degree on the meridian, we get the arch subtended by 44258,7 feet $= 7' \; 16'',4$, which subtracted from the latitude of Beachy Head, namely, $50° \; 44' \; 23'',71$, leaves $50° \; 37' \; 7'',31$ for the latitude of Dunnose.

We have therefore, for finding the length of the degree of a great circle perpendicular to the meridian at Beachy Head, or Dunnose, the latitudes of the two stations, and the angles which those stations make with each other and the pole.

Now it is proved in the Philosophical Transactions, Vol. LXXX. that the sum of the horizontal angles (such as PDB, PBD, fig. 1.) on a spheroid, is nearly the same as the sum of those which would be observed on a sphere, the latitudes, and

also the difference of longitude being the same on both figures. We therefore shall have recourse to that determination, and apply it to the present question.

The co-latitudes of D and B, or the arches D P and B P, are 39° 22′ 52″,69, and 39° 15′ 36″,29, therefore half their sum is 39° 19′ 14″,49, and half their difference 3′ 38″,2.

Half the sum of the angles PDB and PBD is 89° 26′ 25″,5; therefore, as *tang.* 39° 19′ 14″,49 : *tang.* 3′ 38″,2 : : *tang.* 89° 26′ 25″,5 : *tang.* 7° 31′ 57″,71, or half the difference of the angles: hence the angles for computation are 81° 54′ 27″,79, and 96° 58′ 23″,21, which, with the co-latitudes of D and B, give the difference of longitude between Beachy Head and Dunnose, or the angle DPB = 1° 26′ 47″,93.

We have now two right angled triangles, which may be considered spherical, namely, PBW, and PDR, in which the angle at the pole P is given, and likewise the sides PB and PD; therefore, using these *data*, we find the arc BW = 54′ 56″,21, and the arc DR = 55′ 4″,74.

The chords of the two perpendicular arcs are about $3\frac{1}{2}$ feet less than the arcs themselves; therefore BW = 336119,1 feet, and DR = 336983,5 feet; and by proportioning these arcs to their respective values in fathoms, we get the length of the degree of the great circle perpendicular to the meridian in the middle point between W and B = 61182,8 fathoms, and in the middle point between R and D = 61181,8 fathoms. Therefore 61182,3 fathoms is the length of a degree of the great circle perpendicular to the meridian, in latitude 50° 41′, which is nearly that of the middle point between Beachy Head and Dunnose.

If the horizontal angles, or the directions of the meridians,

have been obtained correctly, the difference of longitude between Beachy Head and Dunnose, as thus found, must be very nearly true; since the difference between the sums of the angles which would be observed on a spheroid and those on a sphere, having the latitudes and the difference of longitude the same on both figures as those places, is so small as scarcely to be computed: and it is easy to perceive, that the distance between the parallels is obtained sufficiently correct, since an error of 15 or 20 feet in that meridional arc, will vary the length of the degree of the great circle but a very small quantity.

It may possibly be imagined, that because the vertical planes at Dunnose and Beachy Head do not coincide, but intersect each other in the right line joining these stations, neither of the two included arcs is the proper distance between them, and that the nearest distance on the surface must fall between these arcs; but it is easy to show, that in the present case, the difference must be almost insensible.

In fig. 4, let B be Beachy Head, and E B P its meridian, and N and M, the points where the verticals from Beachy Head and Dunnose respectively meet the axis P P.

Now it is known, that if the planes of two circles cut each other, the angle of inclination is that formed by their diameters drawn through the middle of the chord, which is the line of intersection. Therefore, if we draw B M, and also conceive D to be Dunnose, and E P its meridian, and join D N; it is evident, that either of the angles N B M, N D M will be the inclination of the planes very nearly, because of the short distance between the stations, and their small difference in latitude. In the ellipsoid we have adopted, the distance M N

is about 62 fathoms, and hence the angle N B M, or N D M, will be found between 2 and 3″. The value of the arc between the stations is about 55′ 30″, and its length 339401 feet; hence the versed sine of half the arc will be 685 feet nearly; now, suppose the versed sines to form an angle of 3″, the greatest distance of the vertical planes on the earth's surface between the stations, will be but about $\frac{1}{10}$ of an inch.

It may also be remarked, that the inclination here determined, is the angle in which the vertical plane at one station cuts the vertical at the other; and therefore no sensible variation can arise in the horizontal angles, on account of the different heights of the stations.

If the figure of the earth be that of an ellipsoid, (fig. 5.) then B R, which is perpendicular to the surface at the point B, is the radius of curvature of the great circle, perpendicular to the meridian at that point; therefore the length of a degree of longitude is obtained by the proportion of the radius to the cosine of the latitude. Thus at Beachy Head, where the length of the degree of a great circle is 61183 fathoms nearly, we have this proportion; *rad.* : *cosine* 50° 44′ 24″ : : 61183 : 38718 fathoms, for the length of the degree of longitude. And at Dunnose, as *rad.* : *cosine* 50° 37′ 7″ : : 61182 : 38818 fathoms for the length of the degree of longitude, being about 100 different from the former. But nearly the same conclusions may be otherwise deduced; for the chords of the parallels may be found from the small triangles B W L and D E R, (fig. 3.) and these, when augmented by the differences between them and the arcs, give the length of the degree of longitude at Beachy Head 38719 fathoms, and Dunnose 38819 fathoms.

ART. IV. PROBLEM.

Having given the length of a degree of a great circle perpendicular to the meridian, in the latitude whose tangent is t, and cosine s, and likewise the length of a degree upon the meridian, to find the diameters of the earth, supposing it an ellipsoid.

In fig. 5. let A P A P be the elliptical meridian, passing through the point B, the tangent of its latitude being t, and cosine s; and put $AC = T$, $CP = C$, $D =$ the length of the degree of the great circle, $d =$ that of the degree upon the meridian, and $r = 57°,29$ &c. the degrees in radius. Then, if B F, and A F be the ordinate and abscissa to the point B;

$$FC = \frac{T^2}{\sqrt{T^2 + t^2C^2}},$$

And $\left\{ \begin{array}{l} rD = \dfrac{T^2}{s\sqrt{T^2 + t^2C^2}} = BR, \text{ the radius of curvature of the great circle,} \\ rd = \dfrac{C^2T^2}{s\sqrt{T^2 + t^2C^2}^{\,3}}, \text{ the radius of curvature of the meridional degree.} \end{array} \right.$

These equations give $DC^2 = ds^2 . \overline{T^2 + t^2C^2}$; hence $C = sT\sqrt{\frac{d}{D - dt^2s^2}}$; therefore $C : T :: \sqrt{d} : \sqrt{D + \overline{D - d}.t^2}$, which call as $1 : m$; then $rD = \frac{m^2C}{s\sqrt{m^2 + t^2}}$; and $C = \frac{s\,rD\sqrt{m^2 + t^2}}{m^2}$; therefore T may readily be found.

ART. V. Table, containing a Comparison between the Degrees upon the Meridian, which have been measured in different Latitudes, with those computed on three Ellipsoids whose Magnitudes have been determined by *data* applied to the Conclusions derived from the foregoing Problem.

				1st. Ellipsoid.		2d. Ellipsoid.		3d. Ellipsoid.	
Deg. on meridian in lat. 50° 41′ -				60851 fath.		60870		60851	
Deg. perp. to meridian -				61182		61182		61191	
	Lat. °	′	Measured Fath.	Computed.	Diff.	Computed.	Diff.	Computed.	Diff.
Bouguer, &c. - -	0	0	60482	60122	−360	60183	−299	60103	−379
Mason and Dixon -	39	12	60628	60607	− 21	60640	+ 12	60600	− 28
Boscovich, &c.	43	0	60725	60687	− 38	60716	− 9	60683	− 42
Cassini - -	45	0	60778	60730	− 48	60756	− 22	60727	− 51
Leisganig -	48	43	60839	60806	− 30	60831	− 8	60808	− 31
Betw. Green. and Paris	51	41	60851	60851	0	60870	+ 19	60851	0
Maupertuis, &c. -	60	20	61194	61148	− 46	61150	− 44	61156	− 38

The contents of the above table are computed from the *data* expressed in the different columns at top. In the third column, 60851 fathoms is nearly the length of the degree upon the meridian, as derived by the application of the measured arc between Greenwich and Paris to the difference of latitude, namely, 2° 38′ 26″. The fifth, contains the degrees on an ellipsoid, computed from a different length of a degree upon the meridian in lat. 50° 41′, in order to show how far the varying the length of that degree, will affect the comparison between the measured and computed degrees on the first ellipsoid: and those in the seventh are determined by using 60851 fathoms for the degree upon the meridian, and 61191 fathoms for that of the great circle perpendicular to it; which last degree is obtained by taking the angle at Dunnose, equal to 81° 56′ 53″,5, instead of 81° 56′ 53″.

Now this comparison between the measured and computed degrees, sufficiently proves that the earth is not an ellipsoid,

since the differences are, excepting two instances, constantly *minus*; this, however, presupposes that the degree of the great circle perpendicular to the meridian in lat. 50° 41′, as we have found it, and likewise the degree upon the meridian arising from the measured arc between Greenwich and Paris, and their difference in latitude, are nearly right. Also, were it of Mr. BOUGUER's figure, the degree of a great circle in lat. 50° 41′ would be 61270 fathoms, which is 88 fathoms greater than we have derived it; we may therefore safely infer, that his hypothesis is more ingenious than true; since it cannot be supposed that the degree, resulting from these observations, is 88 fathoms in defect; but whether the earth be a figure formed by the revolution of a meridian round its axis, upon which the length of the degrees increase according to any law, or one whose meridians are formed by the combination of many different curves, it appears to be certain, that we may consider 61182 fathoms as nearly the length of a degree of a great circle, in latitude 50° 41′, by which we are enabled to settle the longitudes of those places whose situations have been determined in this operation.

The length of the degree, as given by General ROY, from the directions of the meridians at Botley Hill and Goudhurst, is 61248 fathoms, which is 66 fathoms different from this result: but this is not to be considered as extraordinary, since the distance between those places is not more than 23 miles, and the direction very oblique to the meridian. It is an indispensable requisite, that the stations chosen for this purpose be nearly east and west; because if both places were on the same parallel of latitude, the horizontal angles would give the difference of longitude, without adverting to the principle of

the sums of the angles on a sphere and a spheroid being nearly equal, when the places on each have corresponding latitudes, and the same difference of longitude.

Was a degree of a great circle perpendicular to the meridian measured in some place remote from the latitude of 50° 41′, the diameters of the earth, supposing it an ellipsoid, might be determined; for if l = the length of a degree of a great circle perpendicular to the meridian, in the latitude whose sine is s and cosine c, and L = the length of the degree in lat. 50° 41′, a and b being the sine and cosine of that latitude; then will the ratio of the axes be that of $\sqrt{l^2 c^2 - L^2 b^2} : \sqrt{L^2 a^2 - l^2 s^2}$. It is therefore, much to be wished, that such measurements were made in the northern part of Russia, and in the south of France, where the methods we have taken to measure this degree would also be applicable.

Having given the length of a degree of what may be considered a great circle upon the earth's surface, as deduced from the observations which have been made at Beachy Head and Dunnose, and likewise drawn such conclusions as appear to arise from it; we shall close this section with observing, that as the preserving of the points marking these stations has been considered of great consequence, his Grace the Duke of RICHMOND ordered an iron gun to be inserted in the ground at each of those places, which was done in the autumn of 1794. By these points being rendered permanent, the truth of this part of the operation can be examined, by re-observing the directions of the meridians; and that this may be done with the least trouble, we have preserved the points, where the staffs were erected on Brading Down and the Hill above Jevington, by inserting large stones in the ground, having a small hole in

each of them, for the purpose of denoting the exact points over which the centres of the staffs were placed; therefore the angles which we have given, as being the directions of the meridians with respect to those points, can be examined without the trouble of firing lights at Beachy Head and Dunnose. There is, however, another method of determining whether 61182 fathoms be nearly the length of a degree of a great circle upon the earth's surface; this may be done by observing the directions of the meridians at Shooter's Hill and Nettlebed, whose distance is already determined, being 242731 feet nearly. The points marking these stations are not likely to be soon removed, and can be found without difficulty.

SECTION SIXTH.

Of the Distances of the Stations from the Meridians of Greenwich, Beachy Head, *and* Dunnose; *and also from the Perpendiculars to those Meridians.*

ART. I.

In operations of this kind, the usual method of obtaining the distances of the stations from a first meridian, and from a perpendicular to that meridian, is by drawing parallels to those lines through the several stations, and then proceeding in a manner similar to that of working a traverse, after the bearings of the stations, with respect to those parallels, have been deduced from the angles of the triangles. This mode of computation might be considered as accurate, if the surface of the earth to the whole extent of the triangles was reduced to a flat: and it will not produce very erroneous results, if the series

of triangles are in a north and south, or an east and west direction nearly, provided they are on, or near the meridian, or its perpendicular; but if the triangles are considerably extended, and in all directions, the bearings of the same stations (if they may be so termed) must evidently differ, and that sometimes considerably, when obtained from different triangles. To avoid, in a great measure, the errors which might affect the conclusions derived from the present triangles, if all those distances were determined from the meridian of Greenwich only, we have considered the meridians of Beachy Head and Dunnose as first meridians also, and, with two or three exceptions, calculated the distance of each station from its nearest meridian. Bagshot Heath, Leith Hill, Ditchling Beacon, and Beachy Head, with those to the eastward, are from the meridian of Greenwich and its perpendicular; Chanctonbury Ring from the meridian of Beachy Head; and the others to the westward, from that of Dunnose.

The advantages in this mode of proceeding are very obvious; for if the directions of meridians are taken at about 80 miles distance from each other, near the southern coast, the operation may be extended to the Land's End with sufficient accuracy, without making astronomical observations for determining any intermediate latitude, as a new point of departure.

In deducing the bearings of the several stations from the meridians and their perpendiculars, we have taken the observed angles, instead of those formed by the chords, which were used in computing the sides of the principal triangles; because the latter angles at each station may be considered as constituting the vertex of a pyramid, and consequently their sum is less than 360°; but the operation of determining the distances

from the meridians, and their perpendiculars from those reduced, or pyramidical angles and the chords or sides of the triangles, independent of other *data*, would be very tedious. Great accuracy however, in these cases seems not absolutely necessary; because, if the latitudes and longitudes obtained from those distances can be depended upon to $\frac{1}{4}$ of a second (the latitude of Greenwich, from which the other latitudes are derived, being supposed exact), the conclusions will certainly be considered as sufficiently near the truth: 25 feet answers to about $\frac{1}{4}$ of a second on the meridian; and it is not difficult to show, that no uncertainty of more than about 10 feet has been introduced, even in the longest distances, in consequence of using the observed angles.

As Botley Hill is nearly south of the Observatory at Greenwich, and it may be supposed, that the distance of it from the meridian, as well as perpendicular, must be nearly true, as given in the Philosophical Transactions, it has not been considered as expedient to make this part of the operation entirely independent of General ROY's, by selecting Greenwich for a station, and observing the direction of the meridian at that place with respect to Banstead, or Shooter's Hill.

In order, therefore, to obtain the necessary *data*, when the instrument was at Botley Hill, the angle between Banstead and the station on Wrotham Hill was observed, as given in a former part of this work, and found to be 152° 57′ 4″,25; from which subtracting 79° 16′ 28″,75, the angle which Wrotham Hill makes with the parallel to the meridian of Greenwich, (Phil. Trans. Vol. LXXX. p. 601.) we get 73° 40′ 35″,5 for the inclination of Banstead to that parallel; this, with 50927 feet, the distance from Banstead to Botley Hill, give 48874,2 feet,

and 14313,5 feet; therefore 48874,2 — 171,5 = 48702,7 feet, is the distance of Banstead from the meridian of Greenwich; and 72881,3 — 14313,5 = 58567,8 feet for the distance from the perpendicular: but it must be remarked, that 171,6 and 72882,5 (see the table of general results, Phil. Trans. Vol. LXXX.) are reduced to 171,5 and 72881,3 feet, by using the proportion of 274047 : 27404,2, the results of the two measurements on Hounslow Heath.

ART. II. Table, containing the Bearings of the Stations from the Parallels to the different Meridians; and likewise their Distances from those Meridians and their Perpendiculars.

Names of stations.		Bearings.		Distance from the Meridian.	Distance from the Perpendicular.
	Meridian of Greenwich.	° ′ ″		Feet.	Feet.
	Botley Hill - -	—	—	171,5	72881,3
Botley Hill - -	Shooter's Hill - -	11 59 23	NE	14899	3533
Botley Hill - -	Banstead - -	73 40 35	NW	48702	58568
Botley Hill - -	Leith Hill - -	66 31 22	SW	84792	109784
Botley Hill - -	Crowborough Beacon	23 3 39	SE	35227	155222
Hanger Hill -	Hampton Poor House	24 11 47	SW	83084	18540
Banstead - -	Hanger Hill -	13 49 33	NW	67234	16733
Banstead - -	King's Arbour -	41 56 31	NW	102261	1036
Banstead - -	St. Ann's Hill -	67 12 13	NW	119400	28854
Crowborough Beacon	Ditchling Beacon -	47 19 22	SW	24468	210257
Leith Hill - -	Ditchling Beacon -	30 58 49	SE	24468	210257
Crowborough Beacon	Brightling -	57 43 12	SE	87304	188119
Brightling - -	Fairlight Down -	61 25 47	SE	143312	218618
Ditchling Beacon -	Beachy Head -	54 39 48	SE	58848	269328
St. Ann's Hill -	Bagshot Heath -	77 27 16	SW	165234	39055
	Merid. of Beachy Head.				
Beachy Head - -	Chanctonbury Ring -	68 26 28	NW	146567	57908
	Meridian of Dunnose.				
Dunnose - -	Rook's Hill - -	45 42 55	NE	102770	100236
Dunnose - -	Butser Hill -	20 58 39	NE	50328	131263
Dunnose - -	Dean Hill - -	34 44 27	NW	104568	150786
Dunnose - -	Motteston Down	73 35 8	NW	52858	15572
Dunnose - -	Nine Barrow Down	87 56 55	NW	188061	6736
Butser Hill -	Highclere -	34 20 17	NW	33174	253495
Dean Hill - -	Highclere -	34 48 11	NE	33174	253495
Dean Hill - -	Beacon Hill -	15 30 36	NW	120101	206757
Dean Hill - -	Four Mile-stone -	54 59 39	NW	151073	183355
Beacon Hill -	Thorney Down -	4 57 42	SE	117871	179212
Beacon Hill -	Old Sarum - -	28 55 42	SW	137793	174746
Dean Hill - -	Wingreen - -	81 32 37	SW	209505	135184
Nine Barrow Down	Wingreen - -	9 28 43	NW	209505	135184
Rook's Hill - -	Hind Head - -	5 43 21	NE	110942	181782

ART. III. *Latitudes and Longitudes of the Stations referred to the Meridian of* Greenwich.

Names of stations.	Latitude.	Longitude. In degrees.		Longitude. In time.
	° ′ ″	° ′ ″		m. s.
Shooter's Hill - -	51 28 5,1	0 3 54,5	E	0 15,6
Crowborough Beacon -	51 3 9,4	0 9 9,5	E	0 36,6
Brightling - -	50 57 43,3	0 22 39,3	E	1 30,6
Fairlight Down -	50 52 38,8	0 37 7,4	E	2 28,5
Beachy Head -	50 44 23,7	0 15 11,9	E	1 0,7
Ditchling Beacon -	50 54 7	0 6 20,5	W	0 25,3
Leith Hill - -	51 10 35,7	0 22 6,3	W	1 28,4
Banstead - -	51 19 2	0 12 44,1	W	0 50,9
Hanger Hill - -	51 31 23,7	0 17 39,6	W	1 10,6
Hampton Poor House -	51 25 35,2	0 21 46,6	W	1 27,1
King's Arbour -	51 28 47,1	0 26 50	W	1 47,3
St. Ann's Hill - -	51 23 51,4	0 31 16,6	W	2 5,1
Bagshot Heath -	51 22 7,1	0 43 15,4	W	2 53

ART. IV. *Latitude and Longitude of Chanctonbury Ring.*

	° ′ ″		m. s.
Lat. of Chanctonbury Ring -	50 53 48,5		
Long. of Beachy Head, east of Greenwich - -	0 15 11,9		
Long. of Chanctonbury Ring, west of Beachy Head -	0 37 58,8		
Long. of Chanctonbury Ring, west of Greenwich -	0 22 46,9	— in time	1 31,1

ART. V. *Latitude and Longitude of* Dunnose.

	°	′	″	
Latitude of Beachy Head -	50	44	23,7	
And taking 60851 fathoms for the length of the degree upon the meridian, we get 44259 feet, the distance between the parallels of Beachy Head and Dunnose - -	0	7	16,4	
	50	37	7,3	lat. of Dunnose.
The difference of long. between Beachy Head and Dunnose has been found in the preceding section -	1	26	47,9	W
And the long. of Beachy Head, east of Greenwich -	0	15	11,9	E
Therefore the long. of Dunnose, west of Greenwich, is	1	11	36	and in time 4 m. 46,4 s.

ART. VI. *Latitudes and Longitudes of the Stations referred to the Meridian of* Dunnose.

Names of stations.	Latitude.			Longitude from Dunnose.				Longitude West of Greenwich. In degrees.			In time.	
	°	′	″	°	′	″		°	′	″	m.	s.
Rook's Hill -	50	53	32,5	0	26	37,7	E	0	44	58,3	2	59,9
Hind Head	51	6	56,1	0	28	53	E	0	42	43	2	50,9
Butser Hill -	50	58	40,8	0	13	3,8	E	0	58	32,2	3	54,1
Mottest. Down	50	39	40	0	13	37,8	W	1	25	13,8	5	40,9
Highclere -	51	18	46,2	0	8	40,4	W	1	20	16,4	5	21,1
Dean Hill -	51	1	50,9	0	27	10,5	W	1	38	46,5	6	35,1
Beacon Hill	51	11	4,4	0	31	18,9	W	1	42	54,9	6	51,7
Four M. stone	51	7	8,5	0	39	20,2	W	1	50	56,2	7	23,8
Thorney D.	51	6	30,2	0	30	40,8	W	1	42	16,8	6	49,1
Old Sarum -	51	5	44,7	0	35	51,5	W	1	47	27,5	7	9,9
Nine B. Down	50	38	3,5	0	48	27,8	W	2	0	3,8	8	0,3
Wingreen -	50	59	7,6	0	54	22,9	W	2	5	58,9	8	23,9

ART. VII.

The longitudes and latitudes of the stations have been computed spherically, in which we have taken the degrees upon the meridian, and of the great circle perpendicular to it, from the following table.

				Degrees on the merid. Fath.	Degrees on the perp. Fath.		Fathoms.
	°	′	″				
Lat.	50	41		60851	61182	Semi-transverse of this ellipsoid -	3491420
	51	5		60859	61185		
	51	28	40	60868	61188	Semi-conjugate -	3468007

Ratio of the axes 1 : 1,006751

This ellipsoid is determined from the length of the degree obtained from the directions of the meridians at Beachy Head and Dunnose, and that upon the meridian in lat. 50° 41′, as resulting from the application of the measured arc between Greenwich and Paris, to their difference in latitude. It is not however, to be understood, that by using it, we consider the earth to be this ellipsoid: we have adopted the hypothesis, because it is obvious some small increase northward must be made to the degree upon the meridian in 50° 41′, in order to approximate to a correct scale for the computation of the latitudes. But it is evident, that any of the received hypotheses (supposing the length of the degree upon the meridian in 50° 41′ to be 60851 fathoms) would give the degrees sufficiently correct, since the principal stations, together with most of the objects fixed in this operation, are included between the parallels of 50° 37′ and 51° 28′.

In obtaining the latitudes of those places which are referred to the meridian of Greenwich, it is easy to perceive, that little error is introduced by spherical computation, since the sphe-

roidical correction for the latitude of Bagshot Heath is only about $\frac{1}{100}$ of a second. Had indeed the latitudes of the stations, which are far to the westward, been computed with distances from the meridian, and the perpendicular at Greenwich, some small errors might have been introduced, from the uncertainty of the earth's figure, and the consequent inability of computing the spheroidical correction with sufficient accuracy; but as the distance between the parallels of Beachy Head and Dunnose is obtained very nearly, the latitude of the latter station may be considered as correct as that of the former one, and consequently the places in the vicinity of Dunnose have their latitudes determined with sufficient precision.

SECTION SEVENTH.

Containing the secondary Triangles, in which two Angles only have been observed. The first seven intersected Places are intended for the small Instrument, on Account of their commanding Situations.

Beachy Head from Ditchling Beacon 102132,4 Feet.

No.	Triangles.	Angles observed.	Distances of the stations from the point intersected.	
		° ′ ″		Feet.
1	Beachy Head -	10 19 30	Firle Beacon -	47956
	Ditchling Beacon	8 53 23		55621
	Firle Beacon -		*Sussex*	

Chanctonbury Ring from the support of High Down Windmill 29442 feet.

2	Chanctonbury Ring	64 54 52	Sleep Down -	17637
	High Down Windmill			27159
	Sleep Down -	79 3 33		

Butser Hill from Rook's Hill 60933,8 feet.

3	Butser Hill - -	10 28 4	Bow Hill -	46150
	Rook's Hill -	28 19 50		17668
	Bow Hill			
4	Butser Hill -	93 25 15	Portsdown Hill	52729
	Rook's Hill -	39 23 59		82926
	Portsdown Hill		*Hampshire*	

Dunnose from Motteston Down 55104,3 feet.

5	Dunnose - -	30 34 9	Thorness -	57470
	Motteston Down	79 6 47		29764
	Thorness		*Isle of Wight*	

Motteston Down from Nine Barrow Down 135489,6.

No.	Triangles.	Angles observed.	Distances of the stations from the point intersected.	
		° ′ ″		Feet.
6	Motteston Down - Nine Barrow Down *Ramsden Hill*	27 57 12 42 26 2	Ramsden Hill - *Hampshire*	97051 67423

Dean Hill from Beacon Hill 58086,3 feet.

7	Dean Hill - Beacon Hill - *Stockbridge Hill*	71 10 48 51 45 47	Stockbridge Hill	54366 65515

With respect to these triangles, there is nothing to be remarked, except that the angles of the 1st and 3d, from their being very acute, were determined with considerable care: the distances however, from Firle Beacon to Ditchling Beacon, and Beachy Head, may be ascertained, when either the great or small instrument are taken to that station, by the intersection of Hurstmonceux Spire.

Triangles formed by the Intersections of Churches, Windmills, and other Objects.

Fairlight Down from Brightling 63773,1 feet.

No.	Triangles.	Angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
1	Fairlight Down - Brightling - *Bexhill Church* -	48 18 18 32 6 22	Bexhill church - *Sussex*	34375 48294

No.	Triangles.	Angles observed. °	′	″	Distances of the stations from the intersected objects.	Feet.
2	Fairlight Down -	46	56	7	Westham Church	70511
	Brightling - -	73	7	30		53832
	Westham Church					
3	Fairlight Down -	46	46	20	Pevensey Church	68526
	Brightling -	71	21	47		52694
	Pevensey Church					
4	Fairlight Down -	4	34	13	Blackheath Wind-mill -	76733
	Brightling -	154	19	13		14110
	Blackheath Windmill					
5	Fairlight Down -	25	26	4	Ninefield Church	45493
	Brightling - -	40	43	54		29943
	Ninefield Church					
6	Fairlight Down -	10	32	37	Mountfield Church	40071
	Brightling -	16	44	22		25458
	Mountfield Church					
6 *	Beachy Head -	76	6	36	Hurstmonceux Church -	47021
	Ditchling Beacon -	26	40	41		101668
	Hurstmonceux Church					

Ditchling Beacon from Crowborough Beacon 81192,2 feet.

No.	Triangles.	°	′	″	Distances	Feet.
7	Ditchling Beacon -	41	17	30	Chittingly Church	69950
	Crowborough Beacon	58	11	13		54320
	Chittingly Church					
8	Ditchling Beacon -	13	23	46	Waldron Church	75316
D	Crowborough Beacon	65	34	25		19165
	Waldron Church					

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
9	Ditchling Beacon - Crowborough Beacon *Firle Church*	67° 16′ 28″ 36 30 43	Firle Church -	49742 77110
10	Ditchling Beacon - Crowborough Beacon *Jevington Windmill*	70 32 0 58 49 56	Jevington Wind-mill -	89861 99016
11	Ditchling Beacon - Crowborough Beacon *Plumpton Church*	34 14 48 3 37 4	Plumpton Church	8347 74441
12 D	Ditchling Beacon - Crowborough Beacon *Little Horstead Church*	23 34 6 28 0 42	Little Horstead Church -	48670 41436
13	Ditchling Beacon - Crowborough Beacon *Spittal Windmill*	66 41 33 14 29 24	Spittal Windmill	20558 75458
14	Ditchling Beacon - Crowborough Beacon *Ditchling Church*	61 49 49 4 48 36	Ditchling Church	7416 77966

Chanctonbury Ring from Ditchling Beacon 63469,1 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
15	Chanctonbury Ring Ditchling Beacon - *Thakeham Church*	115 19 36 13 56 34	Thakeham Church	19754 74103
16	Chanctonbury Ring Ditchling Beacon - *West Grinsted Church*	66 23 40 28 9 20	West Grinsted Church -	30044 58342

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
		°	′	″		
17	Chanctonbury Ring	6	40	15	Keymer Church	59208
	Ditchling Beacon -	55	52	17		8309
	Keymer Church					
18	Chanctonbury Ring	37	47	12	Bolney Church	53461
	Ditchling Beacon -	57	3	58		39029
	Bolney Church					
19 D	Chanctonbury Ring	50	26	25	Slaugham Church	65501
	Ditchling Beacon -	66	41	45		54985
	Slaugham Church					
20	Chanctonbury Ring	23	2	19	Starting House	66986
	Ditchling Beacon -	86	0	59		26279
	Starting House on the Race Ground near Brighthelmstone.					
21	Chanctonbury Ring	33	58	20	Cuckfield Spire	67789
	Ditchling Beacon -	72	9	49		38568
	Cuckfield Spire					
22	Chanctonbury Ring	20	34	55	Wyvelsfield Chur.	71575
	Ditchling Beacon -	98	0	8		25409
	Wyvelsfield Church					
23	Chanctonbury Ring	14	32	35	Hurstpierpoint Church - -	48545
	Ditchling Beacon -	36	29	25		20498
	Hurstpierpoint Church					
24 D	Chanctonbury Ring	29	51	47	Lindfield Church	82079
	Ditchling Beacon -	100	41	5		41590
	Lindfield Church					

Chanctonbury Ring from Sleep Down, 17637 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
25	Chanctonbury Ring Sleep Down - *Goring Church*	° ′ ″ 52 22 45 96 27 23	Goring Church	33866 26995
26	Chanctonbury Ring Sleep Down - *Southwick Church*	22 46 56 140 53 45	Southwick Church	39584 24302
27	Chanctonbury Ring Sleep Down - *Shoreham Church*	14 28 30 151 0 0	Shoreham Church	34094 17578
28	Chanctonbury Ring Sleep Down - *Brighthelmst. Church*	32 5 47 136 19 20	Brighthelmstone Church -	60672 46680
29	Chanctonbury Ring Sleep Down - *Bramber Windmill*	43 9 25 83 16 48	Bramber Windmill - -	21772 14995
30	Chanctonbury Ring Sleep Down - *Temple in Findon Park*	88 47 22 37 32 41	Temple in Findon Park - -	13341 21889

Chanctonbury Ring from Rook's Hill, 85645,4 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
31	Chanctonbury Ring Rook's Hill - - *West Tarring Church*	82 19 10 17 41 21	West Tarring Church -	26426 86189

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
32	Chanctonbury Ring Rook's Hill - - *High Down Windmill*	56 47 5 19 30 39	High Down Windmill - -	29442 73752
33 D	Chanctonbury Ring Rook's Hill - - *Angmering Church*	45 44 35 21 55 49	Angmering Church -	34579 66312
34	Chanctonbury Ring Rook's Hill - *Sir R. Hotham's Flagstaff, near Bersted*	30 40 1 68 36 53	Sir R. Hotham's Flagstaff -	80807 44263
35	Chanctonbury Ring Rook's Hill - - *Bersted Church*	27 54 15 64 26 6	Bersted Church	77325 40115
36	Chanctonbury Ring Rook's Hill - - *Felpham Windmill*	31 22 33 60 52 32	Felpham Windmill - -	74875 44626
37 D	Chanctonbury Ring Rook's Hill - - *Clapham Church*	44 29 25 16 3 16	Clapham Church	27201 68929
38	Chanctonbury Ring Rook's Hill - - *Oving Church*	14 12 22 71 6 26	Oving Church -	81303 21089
39	Chanctonbury Ring Rook's Hill - - *Pagham Church*	27 31 18 89 41 40	Pagham Church	96306 44502

Butser Hill from Rook's Hill 60933,8 feet.

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
		°	′	″		
40	Butser Hill - -	26	55	45	Ower Rocks -	134605
	Rook's Hill - -	134	6	0		84889
	Lantern of the Vessel moored over the Ower Rocks					
41	Butser Hill - -	27	45	25	Selsea Church -	95276
	Rook's Hill - -	117	47	2		50154
	Selsea Church					
42	Butser Hill - -	34	42	20	Selsea High House	99290
	Rook's Hill - -	110	6	12		60199
	Selsea High House					
43	Butser Hill - -	34	40	45	Selsea Windmill	97545
	Rook's Hill - -	109	9	31		58756
	Selsea Windmill					
44	Butser Hill - -	43	21	26	Cackham Tower	77835
	Rook's Hill - -	85	21	20		53613
	Cackham Tower					
45	Butser Hill - -	32	2	23	Bosham Church	61061
	Rook's Hill - -	74	11	15		33667
	Bosham Church					
46	Butser Hill - -	43	28	50	Princested Wind-mill - -	52354
	Rook's Hill - -	57	30	20		42712
	Princested Windmill					

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
47	Butser Hill - - Rook's Hill - - *Del Key Windmill* -	25 41 30 92 32 2	Del Key Windmill	69090 29981
48	Butser Hill - - Rook's Hill - - *West Thorney Church*	43 30 10 68 27 23	West Thorney Church -	61110 45227
49	Butser Hill - - Rook's Hill - - *South Hayling Church*	58 31 52 65 13 29	South Hayling Church -	66544 62510
50	Butser Hill - - Rook's Hill - - *Bourn Church* -	43 27 20 46 55 22	Bourn Church -	44509 41911
51	Butser Hill - - Rook's Hill - - *Flagstaff at the Watch-house near Chichester Harbour*	49 48 19 75 49 16	Flagstaff -	72681 57262
52	Butser Hill - - Rook's Hill - - *Clark's Folly* - -	69 28 9 44 0 16	Clark's Folly -	46151 62212
53	Butser Hill - - Rook's Hill - - *Portsdown Windmill*	83 38 24 41 29 17	Portsdown Wind-mill -	49356 74045
54	Butser Hill - - Rook's Hill - - *West Chimney on the Governor's House, Cumberland Fort.*	69 19 25 61 5 43	Cumberland Fort	70049 74863

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
55	Butser Hill - - Rook's Hill - - *South Sea Castle*	78° 14′ 54″ 59 2 32	South Sea Castle	77038 87953
56	Butser Hill - - Rook's Hill - *St. Cath. Light House*	87 18 4 71 26 30	St. Catherine's Light House - *Isle of Wight*	159328 167881
57	Butser Hill - - Rook's Hill - *Sir R. Worsley's Obelisk*	84 30 52 72 3 59	Sir R. Worsley's Obelisk - *Isle of Wight*	145861 152608
58	Butser Hill - - Rook's Hill - *Ashey Down Sea Mark*	83 29 28 67 44 36	Ashey Down Sea Mark - - *Isle of Wight*	117188 125806
59	Butser Hill - - Rook's Hill - *Flagstaff of Cowes Fort*	103 12 19 50 10 44	Flagstaff, Cowes Fort - - *Isle of Wight*	104463 132415
60	Butser Hill - - Rook's Hill - *Summer House of the Horse-shoe Inn above Cowes*	100 21 10 54 17 51	Summer House -	115573 140005
61	Butser Hill - - Rook's Hill - *Needles Light House*	109 32 45 54 19 57	Needles Light House - - *Isle of Wight*	178277 206796
61*	Butser Hill - - Dean Hill - *Southampton Spire*	23 25 47 32 58 47	Southampton Spire - -	102010 74522

Rook's Hill from Bow Hill 17668 feet.

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
		°	′	″		
62	Rook's Hill - -	132	28	11	Box Grove Church	15306
	Bow Hill - -	21	57	31		30194
	Box Grove Church					
63	Rook's Hill - -	87	10	9	Portfield Windmill	18462
	Bow Hill - -	47	44	17		24916
	Portfield Windmill					
64	Rook's Hill - -	116	1	21	Goodwood House	7938
	Bow Hill - -	18	38	9		22321
	North-west Chimney on Goodwood House					
65	Rook's Hill - -	75	29	10	Chichester Spire	21345
	Bow Hill - -	59	11	56		24057
	Chichester Spire					
	Rook's Hill from Hind Head 81954,4 feet.					
66	Rook's Hill - -	57	8	41	Sir H. Fetherston-haugh's Tower	38424
	Hind Head -	27	50	34		69110
	Sir H. Fetherston-haugh's Tower					
67	Rook's Hill - -	122	22	23	Windmill near Rook's Hill -	3512
	Hind Head -	2	1	34		83887
	Windmill near Rook's Hill					
68	Rook's Hill - -	53	56	49	Harting Wind-mill - -	36328
	Hind Head -	25	52	2		67319
	Harting Windmill					

Chanctonbury Ring from Hind Head 110774.4 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
		° ′ ″		
69	Chanctonbury Ring Hind Head - *Petworth Spire*	13 43 52 16 16 36	Petworth Spire	62080 52576
70 D	Chanctonbury Ring Hind Head - *Wisborough Green Church* - -	12 50 23 11 28 10	WisboroughGreen Church -	53508 59799
71	Chanctonbury Ring Hind Head - *Kirdford Church*	5 12 39 6 29 12	Kirdford Church	61725 49623
72	Chanctonbury Ring Hind Head - *Billinghurst Church*	24 48 50 16 58 51	Billinghurst Church	48543 69755
73	Chanctonbury Ring Hind Head - *Rusper Church*	59 43 43 47 42 51	Rusper Church	85901 100281

Chanctonbury Ring from Butser Hill 141003 feet.

74	Chanctonbury Ring Butser Hill - *The Earl of Egremont's Tower, near Petworth*	20 22 27 18 0 51	The Earl of Egremont's Tower	70219 79052
75 D	Chanctonbury Ring Butser Hill - *Pulborough Church*	25 12 40 8 5 46	Pulborough Church	36163 109375

Leith Hill from Hind Head 82187,7 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
76	Leith Hill - - Hind Head - *St. Martha's Chapel*	41 32 40 27 9 5	St. Martha's Chapel - - *near Guildford*	40257 58505
77	Leith Hill - - Hind Head - *Euhurst Windmill*	11 39 40 3 49 39	Euhurst Windmill	20544 62206
78	Leith Hill - - Hind Head - *Euhurst Church*	12 25 16 3 27 43	Euhurst Church	18135 64596
79	Leith Hill - - Hind Head - *Norris's Obelisk, Bagsbot Heath*	51 3 46 77 52 38	Norris's Obelisk	103310 82191
80	Leith Hill - - Hind Head - *Horsham Spire*	86 36 23 28 38 34	Horsham Spire	43558 90710
81	Leith Hill - - Hind Head - *Farnham Castle*	24 34 44 101 49 30	Farnham Castle	99948 42474

Leith Hill from Ditchling Beacon 117190,4 feet.

82	Leith Hill - - Ditchling Beacon - *Beddingham Windmill*	7 38 23 152 37 54	Beddingham Windmill	159594 46153

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
83	Leith Hill - - Ditchling Beacon - *Firle Windmill*	9 19 46 149 13 1	Firle Windmill	163984 51942

Leith Hill from Crowborough Beacon 128331,9 feet.

84	Leith Hill - - Crowborough Beacon *West Hoathly Church*	6 9 46 10 22 53	West Hoathly Church	81212 48382

Crowborough Beacon from Fairlight Down 125303 feet.

85	Crowborough Beacon Fairlight Down - *Willington Church*	45 4 32 43 6 42	Willington Church	85678 88764

Crowborough Beacon from Brightling 61597,6 feet.

86	Crowborough Beacon Brightling - *Homehurst Church*	12 21 46 70 18 45	Homehurst Church	58474 13297
87	Crowborough Beacon Brightling - - *Hailsham Church*	37 38 24 85 39 48	Hailsham Church	73490 45009
88	Crowborough Beacon Brightling - - *Dallington Church*	6 25 16 83 32 52	Dallington Church	61208 6889

Crowborough Beacon from Botley Hill, 89492,5 feet.

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
		°	′	″		
89	Crowborough Beacon	31	6	44	East Grinsted Church	44729
	Botley Hill	24	17	45		56173
	East Grinsted Church					
90	Crowborough Beacon	17	4	46	Fairden Tower	49295
	Botley Hill -	18	51	52		44777
	Fairden Tower					
91	Crowborough Beacon	93	16	22	Crowborough Chapel	3220
	Botley Hill -	2	3	11		89734
	Crowborough Chapel					
92	Crowborough Beacon	121	34	38	Rotherfield Spire	15517
	Botley Hill -	7	42	43		98509
	Rotherfield Spire					
93	Crowborough Beacon	137	42	2	Mayfield Spire	27585
	Botley Hill -	9	35	19		111453
	Mayfield Spire					
94	Crowborough Beacon	108	47	35	Bestbeech Windmill	36056
	Botley Hill -	18	39	16		106714
	Bestbeech Windmill					
95	Crowborough Beacon	5	2	39	Tatesfield Church	89897
	Botley Hill -	90	24	37		7904
	Tatesfield Church					

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
		°	′	″		
96	Crowborough Beacon	13	26	7	Godstone Wind-mill	78333
	Botley Hill - -	53	50	7		22544
	Godstone Windmill					

Botley Hill from Leith Hill 92632,2 feet.

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
97	Botley Hill - -	17	5	35	Charlwood Church	68505
D	Leith Hill - -	36	33	33		33804
	Charlwood Church					
98	Botley Hill - -	54	41	39	Evelyn's Obelisk	51051
D	Leith Hill - -	33	25	22		75636
	Evelyn's Obelisk					

Butser Hill from Hind Head 78905,7 feet.

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
99	Butser Hill - -	36	49	10	Petworth Wind-mill -	91054
	Hind Head -	83	42	37		54899
	Petworth Windmill					

Portsdown Hill from Butser Hill 52729 feet.

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
100	Portsdown Hill -	41	34	33	Southwick Church	57710
	Butser Hill - -	4	31	23		48564
	Southwick Church					

Dunnose from Butser Hill 140580,4 feet.

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	Feet.
101	Dunnose - -	67	7	31	Flagstaff, Caris-brook Castle -	36697
	Butser Hill -	14	59	6		130763
	Flagstaff of Carisbrook Castle					

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
102	Dunnose - - Butser Hill - *Halifax Tower*	15 4 28 49 11 35	Halifax Tower -	118122 40586

Portsdown Hill from Dunnose 90007 feet.

103	Portsdown Hill - Dunnose - - *Kingston Church, Portsea Island*	33 53 34 9 20 28	Kingston Church	21328 73274
104 D	Portsdown Hill - Dunnose - - *Horndean Church*	150 33 55 7 45 58	Horndean Church	33430 120320
105	Portsdown Hill - Dunnose - - *Titchfield Church*	72 28 16 18 46 40	Titchfield Church	28980 85848

Dunnose from Motteston Down 55104,3 feet.

106	Dunnose - - Motteston Down - *East Corner of the Roof of the great Boat House at the Back of the Isle of Wight*	12 13 22 35 10 30	Great Boat House	43127 15849

No.	Triangles.	Angles observed.			Distances of the stations from the intersected objects.	
		°	′	″		Feet.
107	Dunnose - - Motteston Down - *Brixton Church, Isle of Wight*	5 25	3 53	4 6	Brixton Church	46795 9437
108	Dunnose - - Motteston Down - *East Cowes Sea Mark, Isle of Wight*	54 62	23 29	57 15	East Cowes Sea Mark - -	54796 50235
109	Dunnose - - Motteston Down - *Luttrell's Folly*	50 82	34 14	24 9	Luttrell's Folly	74424 58020
110	Dunnose - - Motteston Down - *Fawley Church*	48 90	58 32	19 45	Fawley Church	84875 64032
111	Dunnose - - Motteston Down - *Flagstaff, Calshot Cast.*	54 80	43 53	0 17	Flagstaff, Calshot Castle - -	77771 64296
112	Dunnose - - Motteston Down - *Fareham Church*	77 66	13 57	3 30	Fareham Church	86636 91814
113	Dunnose - - Motteston Down - *Porchester Church*	87 57	30 50	58 55	Porchester Church	82086 96863
114	Dunnose - - Motteston Down - *Hamble Church*	56 87	5 4	32 16	Hamble Church	91792 76281

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
115	Dunnose - - Motteston Down - *Hamble Saltern*	56 40 50 84 55 52	Hamble Saltern	88390 74150
116	Dunnose - - Motteston Down - *Gov. Hornsby's House, Centre Pediment.*	57 49 18 82 52 7	Gover. Hornsby's House - -	86309 73621
117	Dunnose - - Motteston Down - *Warblington Church*	106 36 6 48 57 49	Warblington Church -	100482 127600
118	Dunnose - - Motteston Down - *Burzledon Windmill*	58 39 40 89 30 11	Burzledon Wind-mill - -	104462 89225
119	Dunnose - - Motteston Down - *Porchester Castle*	87 8 20 58 16 27	Porchester Castle	82568 96952
120	Dunnose - - Motteston Down - *Havant Church*	104 5 1 50 25 55	Havant Church	98725 124221

Dean Hill from Four Mile-stone 56775 feet.

121	Dean Hill - - Four Mile-stone - *Winterslow Church*	42 54 34 21 6 1	Winterslow Church -	22739 43004

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
122	Dean Hill - - Dunnose - - *Farley Monument*	° ' " 60 20 37 16 44 23	Farley Monument	53239 160029

Motteston Down from Nine Barrow Down 135489,6 feet.

123	Motteston Down - Nine Barrow Down *Hordle Church*	33 29 46 16 13 59	Hordle Church	49640 98000
124	Motteston Down - Nine Barrow Down *Mitford Church*	36 52 51 15 13 46	Mitford Church	45098 103035
125	Motteston Down - Nine Barrow Down *Hurst Light House*	33 17 31 9 49 13	Hurst Light House	33813 108820
125	Motteston Down - Nine Barrow Down *Hurst Castle*	33 32 2 9 48 47	Hurst Castle	33564 109043
126	Motteston Down - Nine Barrow Down *Cupola of Sir J. Doyley's House*	67 18 34 20 13 51	Sir J. Doyley's House	46896 125118
127	Motteston Down - Nine Barrow Down *Milton Church*	34 4 47 23 22 56	Milton Church	63782 90057
128	Motteston Down - Nine Barrow Down *North Chimney on Lord Bute's House*	27 1 16 24 13 18	Lord Bute's House	71283 78937

9 48 47		
67 18 31 20 13 51	Sir J. Doyley's House	
34 4 47 23 22 56	Milton Church	
27 1 16 24 13 18	Lord Bute's House	

132	Nine Barrow Down Black Down - - *Funtingdon Church*	7 54 30 28 11 54	Funtingdon Church	101661 29601
133	Nine Barrow Down Black Down - - *Dorchester Church*	7 54 33 30 35 42	Dorchester Church	103647 28022
133*	Nine Barrow Down Black Down - - *Wyke Church, near Weymouth*	15 28 23 54 29 40	Wyke Church	109854 36002

Nine Barrow Down from Wingreen 130224.5 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
		° ′ ″		
134	Nine Barrow Down Wingreen - - *Obelisk near Milbourn, St. Andrew's*	41 50 35 35 56 53	Obelisk near Milbourn - -	78218 88882
135	Nine Barrow Down Wingreen - - *Mr. Trenchard's Tower near Lytchet*	12 9 2 9 3 17	Mr. Trenchard's Tower - -	56660 75778
136	Nine Barrow Down Wingreen - - *Flagstaff, Mr. Pitt's Factory, Isle of Purbeck*	113 10 7 5 30 19	Flagstaff, Mr. Pitt's Factory	14240 136456
137	Nine Barrow Down Wingreen - - *Centre of the Barrow on Creech Hill, Isle of Purbeck*	73 32 14 10 38 14	Barrow on Creech Hill - -	24163 125534
138	Nine Barrow Down Wingreen - - *Vane on the Castle, Branksea Island*	40 45 31 7 37 13	Branksea Castle	23101 113731
139	Nine Barrow Down Wingreen - - *Horton Observatory*	18 12 9 27 4 38	Horton Observatory - -	83424 57250

141 D	Nine Barrow Down Wingreen - - *Ringwood Church*	42 27 24 45 8 30	Ringwood Church	92391 87983
142 D	Nine Barrow Down Wingreen - - *Summer House at Moyle's Court*	41 55 41 53 51 18	Summer House, Moyle's Court	105698 87461
143	Nine Barrow Down Wingreen - - *Christchurch Tower*	66 36 0 29 45 57	Christchurch -	65052 120256
144	Nine Barrow Down Wingreen - - *Warren Summer House, Christchurch Head*	72 43 29 29 13 29	Warren Summer House - -	64989 127104

Motteston Down from Wingreen 197090 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the objects intersected.	
		° ′ ″		Feet.
145 D	Motteston Down - Wingreen - - *Sopley Church*	11 6 40 10 13 47	Sopley Church -	96183 104370

Dean Hill from Beacon Hill 58086,3 feet.

146	Dean Hill - - Beacon Hill - *Salisbury Spire*	53 21 33 35 37 6	Salisbury Spire -	33834 46615

Beacon Hill from Four Mile-stone 38818,2 feet.

147	Beacon Hill - - Four Mile-stone - *Altar-piece at Stone Henge*	33 20 34 34 52 8	Altar-piece at Stone Henge	23900 22978
148	Beacon Hill - - Four Mile-stone - *Amesbury Church*	20 44 17 11 52 14	Amesbury Church	14817 25506
149	Beacon Hill - - Four Mile-stone - *South Chimney on Old Hartford Hut, Salisbury Plain*	16 26 51 56 13 33	Old Hartford Hut	33801 11513
150	Beacon Hill - - Four Mile-stone - *Everley Church*	132 24 37 23 7 41	Everley Church	36822 69215

152	Beacon Hill - - Four Mile-stone - *North Windmill, Salisbury Plain*	45 4 20 81 52 17	North Windmill	48082 34387
153	Beacon Hill - - Four Mile-stone - *South Windmill, Salisbury Plain*	41 55 52 74 6 39	South Windmill	41554 28871

Beacon Hill from Wingreen 114522,4 feet.

154	Beacon Hill - - Wingreen - - *Clay Hill Barrow, near Warminster*	42 46 45 70 18 36	Clay Hill Barrow, or Copt Heap -	117216 84554

Triangles for finding the Distance of Portsmouth Observatory from Dunnose.

Dunnose from Motteston Down 55104,3 feet.

No.	Triangles.	Angles observed.	Distances of the stations from the intersected objects.	Feet.
155	Dunnose - - Motteston Down - *Spindle of the Wind Vane on Portsmouth Church Tower*	° ′ ″ 92 44 48 48 44 27	Portsmouth Church	66524 88393
156	Dunnose - - Motteston Down - *Ball of the Cupola of Portsmouth Academy*	91 35 32 50 43 36	Portsmouth Academy -	69787 90113

In order to ascertain the situation of the Observatory, Mr. Bayly, Master of the Academy, measured two angles in the following triangle, viz.

	°	′	″
Portsmouth Academy	124	9	15
Observatory -	53	6	15
Portsmouth Church			

The included angle at Dunnose between the Ball on the Cupola of the Academy, and the Spindle of the Wind Vane on Portsmouth Church, is 1° 9′ 16″, and the distances of those objects from Dunnose are 66524 and 69787 feet; therefore the distance between the Academy and the Church will be 3540 feet:

Angles observed.	Distances of the station from the intersected objects.
92 44 48 48 44 27	Portsmouth Church
91 35 52 50 43 56	Portsmouth Academy

[illegible] the situation of the Observatory, [illegible] the Academy, measured two angles in the [illegible] viz.

Remarks.

In an operation of this kind, it naturally follows, when the objects intersected are at considerable distances from the stations, there must be great difficulty in ascertaining their precise situations from the appearance of the country. Under such circumstances their names sometimes cannot be discovered; and it has been found, that the best maps of which we are in possession, were by no means sufficiently correct to be of much service in that particular. It is obvious also, without a very intimate knowledge of the interior parts of the country, (of which it is impossible, in the present state of the survey, we can be altogether possessed), there must be some difficulty to identify them, when their distances exceed twelve or fourteen miles. We have, therefore, when such an uncertainty existed, had recourse to some intelligent person well acquainted with the country, by whom we have been informed of their names.

there has been any misnomer; but, as there is not altogether a certainty that all are rightly named, or the objects actually intersected, we have prefixed a D to those we consider as doubtful.

It may be proper to observe, that in taking the angles, the most defined parts of the objects have been selected, unless they were church towers without spires or pyramidical roofs, when the angles were taken to the middles of the towers. If the objects were windmills, resting (as they sometimes do) on great spindles, the observations have been made to those spindles; but in other cases, when the supports were undefined, the mills themselves were intersected.

Westham Church -	11 41 43	SW	76392	240833
Pevensey Church -	9 56 0	SW	78214	240023
Black Heath Windmill	87 6 34	NW	73212	187407
Ninefield Church -	20 41 53	SE	97887	216129
Mountfield Church -	78 10 9	SE	112221	193388
At Ditchling Beacon.				
Chittingly Church -	88 37 2	NE	45462	208569
Waldron Church -	60 43 18	NE	41227	173423
Firle Beacon Station -	63 33 11	SE	25332	235029
Firle Church -	65 24 0	SE	20759	230963
Jevington Windmill -	62 8 28	SE	54978	252248
Plumpton Church -	81 34 20	NE	16211	209034

4 D 2

Bearings from the Parallels to the Meridian.					Distances from merid.	Distances from perp.
	°	′	″		Feet.	Feet.
At Ditchling Beacon.						
Little Horsted Church	70	53	38	NE	21521	194326
Spittal Windmill -	65	58	55	SE	5690	218625
Ditchling Church -	14	30	17	NW	26325	203077
Thakeham Church -	77	33	40	NW	96831	194295
West Grinsted Church	63	20	56	NW	76612	184087
Keymere Church -	35	37	57	NW	29309	203504
Bolney Church - -	34	26	16	NW	46539	178068
Slaugham Church -	24	48	29	NW	47538	160346
Starting House, Brighton	2	28	47	SW	25605	236511
Cuckfield Church -	12	20	25	NW	32711	172580
Wyvelsfield Church -	6	29	54	NE	21592	185011
Hurstpierpoint Church	55	0	49	NW	41262	198504
Lindfield Church -	9	10	51	NE	17832	169199
At Crowboro. Beacon.						
Willington Church -	14	31	53	SE	56724	238159
Homehurst Church -	70	4	58	SE	90204	175142
Hailsham Church -	20	4	48	SE	60458	224245
Dallington Church -	51	17	56	SE	82994	193493
At Botley Hill.						
East Grinsted Church	1	14	6	SW	1039	129041
Rotherfield Church -	30	46	22	SE	50573	157520
Mayfield Church -	32	38	58	SE	60300	166723
Crowborough Chapel	25	6	50	SE	38257	154132
Bestbeech Windmill -	41	42	55	SE	71183	152539
Fairden Tower - -	4	11	47	SE	3448	117538
Tatesfield Church -	66	31	44	NE	7422	69733
Godstone Windmill -	30	46	28	SW	11363	92251
Charlwood Church -	49	25	48	SW	51866	117435
Evelyn's Obelisk - -	11	49	44	SW	10294	122847

Bearings from the Parallels to the Meridian.					Distances from merid.	Distances from perp.
At Butser Hill.	°	′	″		Feet.	Feet.
Pulborough Church -	86	21	25	SE	159482	124313
E. of Egremont's Tower	83	43	30	NE	128906	139903
Bosham Church -	27	20	55	SE	78379	77027
Selsea Church - -	31	37	53	SE	100296	50141
Prinsted Windmill -	15	54	28	SE	64678	80914
Del Key Windmill -	33	41	48	SE	88659	73781
Horse-shoe Summer House - -	41	57	52	SW	26952	45327
Southampton Spire -	73	45	14	SW	47618	102722
Selsea Windmill -	24	42	33	SE	91103	42649
Flagstaff, Chichester Harbour - -	9	34	59	SE	62428	59596
Cackham Tower -	16	1	52	SE	71823	56455
Selsea High House -	24	40	58	SE	91791	41045
Bourn Church -	15	55	58	SE	62546	88464
Ower Rocks - -	32	27	33	SE	122570	17687
South Hayling Church	0	51	26	SE	51324	64727
West Thorney Church	15	53	8	SE	67055	72486
Bow Hill Station -	48	55	14	SE	85116	100937
St. Cath. Light House	27	54	46	SW	24258	9529
Needles Light House	50	9	27	SW	86554	17045
Worsley's Obelisk -	25	7	34	SW	11607	796
Ashey Down Sea Mark	24	6	10	SW	2471	24292
Cowes Fort - -	43	49	1	SW	21998	55887
Portsdown Windmill	24	15	6	SW	30055	86262
Clark's Folly - -	10	4	51	SW	42250	85825
South Sea Castle -	18	51	36	SW	25425	58361
Portsdown Hill Station	34	2	0	SW	20816	87565
Petworth Windmill -	87	0	38	NE	141258	136012
At Rook's Hill.						
West Tarring -	73	52	32	SE	185568	76299
High Down Windmill	72	3	14	SE	172934	77511
Angmering Church -	69	38	4	SE	164937	77159
Pagham Church -	1	25	13	SE	104222	55757

Bearings from the Parallels to the Meridian.					Distances from merid.	Distances from perp.
	°	′	″		Feet.	Feet.
At Dunnose.						
Portsmouth Academy	18	0	24	NE	21573	66369
———— - Observatory	18	6	10	NE	21739	66499
Fareham Church -	3	37	55	NE	5488	86462
Porchester Church -	13	55	50	NE	19762	79672
Havant Church - -	30	29	53	NE	50104	85066
At Dean Hill.						
Salisbury Spire -	63	52	9	NW	136127	162983
Stockbridge Hill Station	55	40	12	NE	59673	181446
Winterslow Church -	12	5	5	NW	69329	173021
At Four Mile-stone.						
North Windmill, Salisbury Plain -	21	11	6	NW	161506	210275
South Windmill, Salisbury Plain - -	28	56	44	NW	167717	213446
At Motteston Down.						
Ramsden Hill Station	65	47	6	NW	141369	55377
Hordle Church -	60	14	32	NW	95952	40210
Mitford Church -	56	51	27	NW	90619	40228
Milton Church -	59	39	31	NW	107904	47792
Hurst Light House -	60	26	47	NW	82272	32250
Hurst Castle - -	60	12	16	NW	81985	32249
Lord Bute's House -	66	43	2	NW	118336	43747
Summer House, Kilminston Down -	23	24	52	NE	3259	145161
Sir J. Doyley's House -	26	25	44	NW	73731	57566
Belvidere House -	64	55	39	NW	117904	46004
Sopley Church -	63	44	46	NW	139119	58118
At Nine Barrow Down.						
Wyke Church - -	84	7	0	SW	297337	4524
Horton Observatory -	8	43	26	NE	175408	89196
Branksea Castle -	31	16	48	NE	176067	26479
Swyre Head - -	65	41	52	SW	208018	2275

place from the parallels to the meridian of Dunnose, and its perpendicular. But the distances of Beacon Hill from that meridian, and perpendicular, are 120101 feet, and 206757 feet; therefore 120101 + 116916 = 237017 feet, and 206757 + 8376 = 215133 feet, are the distances of Clay Hill from the meridian of Dunnose, and its perpendicular.

ART. II. *Containing the Latitudes and Longitudes of such Places upon the Sea Coast, and near it, as have been referred to the Meridian of* Greenwich.

Names of objects.	Latitude.	Longitude from Greenwich. In degrees		In time.
	° ′ ″	° ′ ″		m. s.
Bexhill Church -	50 50 46,7	0 28 43,3	E	1 54,9
Pevensey Church -	50 49 11,9	0 20 14,1	E	1 20,9
Westham Church -	50 49 4	0 19 45,8	E	1 19
Willingdon Church	50 49 31,2	0 14 40,6	E	0 58,7
Jevington Windmill	50 47 12,3	0 14 12,8	E	0 56,9
Firle Beacon Station	50 50 2,7	0 6 33,3	E	0 26,2
Firle Windmill -	50 50 4,8	0 5 30,6	E	0 22
Firle Church - -	50 50 42,9	0 5 22,4	E	0 21,5
Beddingham Windmill	50 50 8,3	0 3 50,1	E	0 15,3
Hailsham Church -	50 51 48,2	0 15 39,3	E	1 2,6
Spittal Windmill -	50 52 44,7	0 1 28,3	W	0 26
Starting House, Brighton - -	50 49 48,1	0 6 28,5	W	0 25,9

Names of objects.	Latitude.	Longitude from Dunnose.	Longitude west of Greenwich. In degrees.	Longitude west of Greenwich. In time.
	° ′ ″	° ′ ″	° ′ ″	m. s.
Cumberland Fort	50 47 20,8	0 9 53,2 E	1 1 43	4 6,9
Kingston Church	50 48 13,5	0 7 19,1 E	1 4 16,9	4 17,1
Havant Church	50 51 5,4	0 12 58,3 E	0 58 37,7	3 54,5
Portsdown Windmill	50 51 17,6	0 7 46,9 E	1 3 49,1	4 15,3
Portsdown Station	50 51 30,6	0 5 23,4 E	1 6 12,6	4 24,8
Portsmouth Church	50 47 26,8	0 5 38,7 E	1 5 57,3	4 23,8
Portsmouth Academy	50 48 1,6	0 5 34,7 E	1 6 1,3	4 24,1
Portsmouth Observatory	50 48 2,9	0 5 37,3 E	1 5 58,7	4 23,9
South Sea Castle	50 46 42,5	0 6 34,3 E	1 5 1,7	4 20,1
Porchester Church	50 50 12,7	0 5 6,9 E	1 6 29,1	4 25,9
Porchester Castle	50 50 18,6	0 5 0,5 E	1 6 35,5	4 26,3
Fareham Church	50 51 19,8	0 1 25,3 E	1 10 10,7	4 40,7
Titchfield Church	50 51 10	0 2 5,6 W	1 13 41,6	4 54,8
Hamble Saltern	50 51 0,9	0 6 39,2 W	1 18 15,2	5 13
Hamble Church	50 51 30,3	0 7 8,6 W	1 18 44,6	5 15
Calshot Castle	50 43 12,7	0 6 29,6 W	1 18 5,6	5 12,4
Luttrell's Folly	50 48 22,5	0 7 31,5 W	1 19 7,5	5 16,5
Fawley Church	50 49 47,7	0 9 8,4 W	1 20 44,4	5 22,9
Hurst Castle	50 42 23,4	0 21 9,5 W	1 32 45,5	6 11
Hurst Light House	50 42 23,4	0 21 14 W	1 32 50	6 11,3
Ashey Down Sea Mark	50 41 6,8	0 0 38,2 E	1 10 57,8	4 43,8
East Cowes Sea Mark	50 45 37,5	0 4 39,2 W	1 16 15,2	5 5
West Cowes Fort	50 46 18,2	0 5 41,1 W	1 17 17,1	5 9,1
St. Catherine's Light House	50 35 33,1	0 6 14,7 W	1 17 50,7	5 11,3
Needles Light House	50 39 53,2	0 22 19,2 W	1 33 55,2	6 15,7
Mitford Church	50 43 41,7	0 23 23,9 W	1 34 59,9	6 20
Milton Church	50 44 55,2	0 27 52,4 W	1 39 28,4	6 37,9
Hordle Church	50 43 41,2	0 24 46,5 W	1 36 22,5	6 25,5
Lord Bute's House	50 44 14,5	0 30 33,7 W	1 42 9,7	6 48,6
Christchurch Head	50 42 57,3	0 33 34,5 W	1 45 10,5	7 0,7
Christchurch Tower	50 43 56,8	0 34 27,4 W	1 46 3,4	7 4,2
Ramsden Hill	50 46 7,5	0 36 32 W	1 48 8	7 12,5
Castle, Branksea Island	50 41 19,5	0 45 25,5 W	1 57 1,5	7 48,1
Poole Church	50 42 50	0 47 18,6 W	1 58 54,6	7 55,6
Flag-staff, Mr. Pitt's Factory	50 36 46,5	0 51 31,9 W	2 3 7,9	8 12,5
Creech Barrow	50 38 9,8	0 54 38,9 W	2 6 14,9	8 25
Barrow, Swyre Head	50 36 32,4	0 53 34,8 W	2 5 10,8	8 20,7
Boat House	50 37 37,9	0 11 4,9 W	1 22 40,9	5 30,7
Wyke Church	50 35 57,5	1 16 34,2 W	2 28 10,2	9 52,7
Brixton Church	50 38 37,6	0 11 49,2 W	1 23 25,2	5 33,7
Horse-shoe Summer House	50 44 34,2	0 6 57,7 W	1 18 33,7	5 14,2
Sir R. Worsley's Obelisk	50 36 59,5	0 2 59,4 W	1 14 35,4	4 58,3
Ower Rocks	50 39 57,3	0 31 36,5 E	0 39 59,5	2 40

ART. VI. Containing the Latitudes and Longitudes of those Places, remote from the Sea Coast, which have been referred to the Meridian of Dunnose.

Names of objects.	Latitude.	Longitude from Dunnose.	Longitude west of Greenwich.	
			In degrees.	In time.
	° ′ ″	° ′ ″	° ′ ″	m. s.
Rusper Church	51 7 22,4	0 54 59,1 E	0 16 36,9	1 6,5
Billinghurst Church	51 1 20,6	0 44 43,9 E	0 26 52,1	1 47,5
Pulborough Church	50 57 25,5	0 41 22,4 E	0 30 13,6	2 0,9
Kirdford Church	51 1 44,1	0 38 49,9 E	0 32 46,1	2 11,1
Petworth Windmill	50 59 22,5	0 36 40,7 E	0 34 53,3	2 19,6
Petworth Church	50 59 17	0 35 10,2 E	0 36 25,8	2 25,7
Earl of Egremont's Tower	51 0 1,9	0 33 28,7 E	0 38 7,3	2 32,5
Wisborough Green Church	51 1 20,1	0 41 41 E	0 29 55	1 59,7
Boxgrove Church	50 51 36,7	0 29 10,1 E	0 42 25,9	2 49,7
Portfield Windmill	50 50 31,4	0 26 10,5 E	0 45 25,5	3 1,7
Rook's Hill Windmill	50 53 17,2	0 25 48,9 E	0 45 47,1	3 3,1
Halifax Tower	50 52 47,5	0 18 0,4 E	0 53 35,6	3 34,4
Goodwood House	50 52 20,8	0 27 26,7 E	0 44 9,3	2 56,6
Bow Hill Station	50 53 40,4	0 22 3,3 E	0 49 32,7	3 18,2
Harting Windmill	50 57 32,7	0 19 38,2 E	0 51 57,8	3 27,8
Sir H. Fetherstonhaugh's Tower	50 57 30,3	0 18 52,3 E	0 52 43,7	3 30,9
Horndean Church	60 55 33,3	0 11 15,1 E	1 0 20,9	4 1,4
Southwick Church	50 52 27	0 5 11,7 E	1 6 24,3	4 25,6
Summer House, Kilminston Down	51 0 58,5	0 0 50,8 E	1 10 45,2	4 43
Carisbrook Castle	50 41 17,5	0 6 49,9 W	1 18 25,9	5 13,7
Bursledon Windmill	50 53 42,3	0 6 58,3 W	1 18 34,3	5 14,3
Thorness Station	50 44 1,1	0 10 7,5 W	1 21 43,5	5 26,9
Farley Monument	51 2 12,8	0 12 54,1 W	1 24 30,1	5 38
Southampton Spire	50 53 59,5	0 12 20,4 W	1 23 56,4	5 35,8
Stockbridge Hill Station	51 6 55,3	0 15 32,2 W	1 27 8,2	5 48,5
Sir J. Doyley's House	50 46 33,3	0 19 3,4 W	1 30 39,4	6 2,6
Winterslow Church	51 5 29,7	0 28 27 W	1 40 3	6 40,2
Belvidere House	50 44 36,9	0 30 27,3 W	1 42 3,3	6 48,2
Everley Church	51 17 3,3	0 30 29,3 W	1 42 5,3	6 48,3
Ringwood Church	50 50 58	0 35 40 W	1 47 16	7 9,1
Summer House, Martincel's Hill	51 22 3,6	0 33 45 W	1 45 21	7 1,4
Summer House, Moyle's Court	50 52 48,2	0 34 1,5 W	1 45 37,5	7 2,5
Amesbury Church	51 16 18,9	0 35 0,8 W	1 46 36,8	7 6,5
Salisbury Spire	51 3 48,9	0 35 24,2 W	1 47 0,2	7 8
Sopley Church	50 46 34,7	0 35 57,5 W	1 47 33,5	7 10,2
Stonehenge	51 10 44,3	0 37 31,8 W	1 49 7,8	7 16,5
Old Hartford Hut	51 9 1,8	0 39 32,1 W	1 51 8,1	7 24,5
S. Windmill on Salisbury Plain	51 11 33	0 42 7,2 W	1 53 43,2	7 34,9
N. Windmill on Salisbury Plain	51 12 3,4	0 43 44,8 W	1 55 20,8	7 41,4
Horton Observatory	50 51 37,9	0 45 25,3 W	1 57 1,3	7 48,1
Mr. Trenchard's Tower	50 46 40,5	0 54 0,5 W	2 5 36,5	8 22,4
Clay Hill, or Copt Heap	51 12 12	1 1 49,8 W	2 13 25,8	8 53,7
Alfred's Tower	51 6 54,4	1 9 45,5 W	2 21 21 5	9 25,4
Milbourn Obelisk	50 45 57,8	1 4 22,8 W	2 15 58.8	9 3,9
Funtingdon Church	50 42 52,2	1 13 35,5 W	2 25 11,5	9 40,7
Dorchester Church	50 42 57,7	1 14 4,1 W	2 25 40,1	9 42,7

At Rook's [illegible] at Butser Hill [illegible]

At Butser [illegible] at Dunnose [illegible]

[illegible] the Top of a Flagstaff on Rook's Hill [illegible]

Dunnose and Rook's Hill [illegible]

Dunnose and Butser Hill [illegible]

Butser Hill and Rook's Hill [illegible]

The Flagstaff on Rook's Hill [illegible]

[illegible]

[illegible] and Rook's Hill will be found [illegible] between Dunnose and Butser Hill [illegible] and between Butser Hill and Rook's Hill [illegible]

Stations.	Ground above low water.
	Feet.
Leith Hill - -	993
Ditchling Beacon -	858
Beachy Head - -	564
Fairlight Down -	599
Brightling Down -	646
Crowborough Beacon -	804
Botley Hill - -	890*
Banstead - - -	576
Shooter's Hill - -	446
Hanger Hill - -	230
King's Arbour - -	118
Hampton Poor House -	86
St. Ann's Hill - -	240
Bagshot Heath -	463
Dean Hill - -	539
Beacon Hill - -	690
Old Sarum - -	266
Nine Barrow Down -	642
Highclere - -	900
Wingreen - -	941
Motteston Down -	698*
Bow Hill - -	702*
Portsdown Hill -	447*

Between	Mean Refraction.
Dean Hill and Wingreen - -	$\frac{1}{15}$ of the contained arc.
Rook's Hill and Butser Hill -	$\frac{1}{15}$
Nine Barrow Down and Wingreen -	$\frac{1}{17}$
Leith Hill and Ditchling Beacon -	$\frac{1}{18}$
Mean of all the above, nearly -	$\frac{1}{12}$
Leith Hill and the Horizon -	$\frac{1}{10}$
Rook's Hill and the Horizon -	$\frac{1}{10}$
Nine Barrow Down and the Horizon	$\frac{1}{10}$

ART. III. *Remarks on the foregoing Tables.*

The height of the ground at the station on St. Ann's Hill, table 1, is 240 feet; but according to General ROY (Phil. Trans. Vol. LXXX. p. 232) it is 321 feet: this very great disagreement, however, principally arises from the variableness in the terrestrial refraction. In 1787, at the station near Hampton Poor House, the ground at St. Ann's Hill was elevated 17′ 39″; but at the same station in 1792, *when the axis of the instrument was at the same height above the ground*, the elevation was only 8′ 11″. General ROY took $\frac{1}{10}$ of the contained arc for the effect of refraction, and considered the height of St. Ann's Hill, when deduced from that of the station near Hampton Poor House, as more accurate than could be obtained by way of the station at the Hundred Acres. But, previous to the survey in 1787, he found by the barometer, that the station on St. Ann's Hill was 200 feet higher than the Thames at Shepperton; and he added 33 feet for the descent to low water at the sea; the sum is 233 feet, agreeing nearly with our determination.

Hill, we found the depression of the ground at Chanctonbury Ring, vary from 1′ 41″ to 2′ 30″. The observations, however, on which the tables are founded, were made in close cloudy days, or toward the evenings, when the tremulous motion in the air is commonly the least.

It has been conjectured, that the variations in terrestrial refraction, depend on the changes in the atmosphere indicated by the barometer and thermometer: this, however, cannot be the case when the rays of light pass near the earth's surface for any considerable distance. M. DE LA LANDE, in his Astronomy (Art. *Terrest. Ref.*), remarks, that the mountains in Corsica are sometimes seen from the coasts of Genoa and Provence, but at other hours on the same days, they totally disappear, or are lost as it were in the sea. And the late General ROY frequently mentioned an instance of extraordinary refraction, which himself and Colonel CALDERWOOD observed on Hounslow Heath, when they were tracing out the base. Their levelling telescope at King's Arbour was directed towards Hampton Poor House, where a flagstaff was erected at that end of the base; this for a long time they endeavoured in vain to discover, till at last, very unexpectedly, it suddenly started up into view, and so high it seemed to be lifted, that the surface of the ground where it stood, became visible. This will appear the more extraordinary, when it is considered, that a right line drawn from the eye at King's Arbour to the other end of the base, would pass 8 or 9 feet below the surface of the intermediate ground near the Duke of ST. ALBANS Park. The following is still more singular. "I observed," says Mr. DALBY, "what seemed to me a very uncommon effect of ter-"restrial refraction, in April, 1793, as I went from Freshwater

not properly driven, till in the afternoon, when we found that the curve appearance was lost, and the ebullition in the air had subsided.

The new raised earth about the gun at King's Arbour, prevented a very accurate measurement of the height of the instrument above the point of commencement of the base; and therefore two opportunities only presented themselves for determining the actual terrestrial refraction; namely, at the ends of the base of verification. From the depression taken at Beacon Hill, the refraction was 38″; but the elevation of Beacon Hill, observed at the lower end, near Old Sarum, gives 50″. These deductions, perhaps, cannot be deemed very conclusive; because, as they depend on the difference in the vertical heights of the ends of the base, every 2 inches of error in that difference will produce an error of about 1″ in the computed refraction. We shall close this section with the *data* whence those refractions were obtained.

At Beacon Hill, the top of the flagstaff near Old Sarum was depressed 42′ 6″.

At the other end of the base, near Old Sarum, the top of the flagstaff at Beacon Hill was elevated 38′ 42″.

The axis of the telescope at Beacon Hill was 15 inches above, and the top of the flagstaff 91 inches above the point where the mensuration began. Near Old Sarum it was 28 inches higher, and the top of that flagstaff 95 inches above where the base terminated. This end (see Sect. III.) is 429,48 feet lower than the other. Lastly, the value of the base is 6′ of a degree, very nearly.

Head, and Wyke Church near Weymouth. Those are some of the principal objects which mark the coast, being very near it.

Upon the commencement of the present business, the design was to divide it into two parts; namely, one for ascertaining distances from the triangles, whose angles were to be observed with the large theodolite; and the other, the interior survey of the country, in which a small instrument, made upon the same plan with the great one, was intended to be used. This instrument being now nearly finished, that design will be carried into execution; and as two or three hundred *single* bearings have been taken from the different stations, which cannot at present be made use of, an important addition will be made to the number of places already fixed, independent of others, whose situations will be determined with it, in the course of the survey. The result of this, as well as the other parts of the trigonometrical operation, will be given to the public, in the Philosophical Transactions. And should it be discovered, from the use of the small instrument, that any of the secondary triangles are erroneous, such errors will be corrected, as well as any *errata* which we may find in this account.

From the instructions given to those who have the honour to be employed in this undertaking, namely, to consider the survey of the sea coast, in the first stage of the business, their principal object, the design is to carry on a series of triangles to the Land's End. For that purpose, there are already five new stations selected; two in the Isle of Portland; one on Charton Common, near Lyme; another on Pilsden Hill, near Broad Windsor, and the other on a hill near Mintern; all in Dorset-

	PRESENTS.	DONORS.
	Syllabus of a Course of Lectures on Botany, by J. E. Smith. London, 1795. 8°	James Edward Smith, M. D. F. R. S
May 14.	Annals of such Patriots of the Family of Fraser, as have signalized themselves in the public Service of Scotland. Edinburgh, 1795. 8°	Hon. Archibald Campbell Fraser, F. R. S.
June 25.	J. F. Blumenbach de Generis Humani Varietate nativa. Editio 3tia. Gottingæ, 1795. 8°	Professor Blumenbach, F. R. S.
	C. Ptolomæus Beobachtung und Beschreibung der Gestirne, mit erläuterungen, von J. E. Bode. Berlin, 1795. 8°	Mr. J. E. Bode, F. R. S.
	Anecdotes of some distinguished Persons. Vol. III. 1795. 8°	William Seward, Esq. F. R. S.

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ERRATA.

PART I.

Page 147 line 13, *for* 25, *read* 2,5.

PART II.

Page 298 line 2, insert *the Rev.* after *By*.
Page 302 line 2, *insert* in *after* units.
Page 318 line 2 from bottom, for $p \times \overline{n+1-mrz}^{n+1}$, read $p \times \overline{n+1-mr}\,z^{n+1}$.
Page 343 line 5 from bottom, for *f, g*, read *c, f.*
line 3 from bottom, for 3. *c*, read 3. *b*.
line 2 from bottom, for *f, g*, read *c, f.*
Page 344 line 3, for *i, k*, read *b, i.*
line 4, for *g*, read *f.*
line last but one, for *g*, read *f.*
Page 345 line 4, for *i*, read *b*.
line 5, for *g*, read *b*.
Page 364 line 6, *for* rail, *read* rails.
Page 372 line 17, *dele* comma *after* shape.
Page 433 line 11, *for* in which the chains were laid off, *read* to which the chains were reduced.
Page 492 line 17, *for* in the degrees, *read* in degrees.
Page 517 line 2, *for* Direction, *read* Directions.
Page 522 line 11, *for* E and L, *read* R and W.
Page 526 line 4, *for* a degree, *read* the degree.
Page 537 line 19, *for* hopothesis, *read* hypothesis.
Page 557 iine 8, *et alibi*, *for* Gov. Hornsby, *read* Gov. Hornby.

..., for *f*, *g*, ...
... *k*, read *h*, *i*.
... *f*.
..., for *g*, read *f*.
..., read *b*.
..., read *h*.
..., read tails.
... after shape.
... the chains were laid off, read to which the chains were
...
... degrees, read in degrees.
..., read Directions.
... L, read R and W.
..., read the degree.
..., read hypothesis.
... Gov. Hornby, read Gov. Hornby.